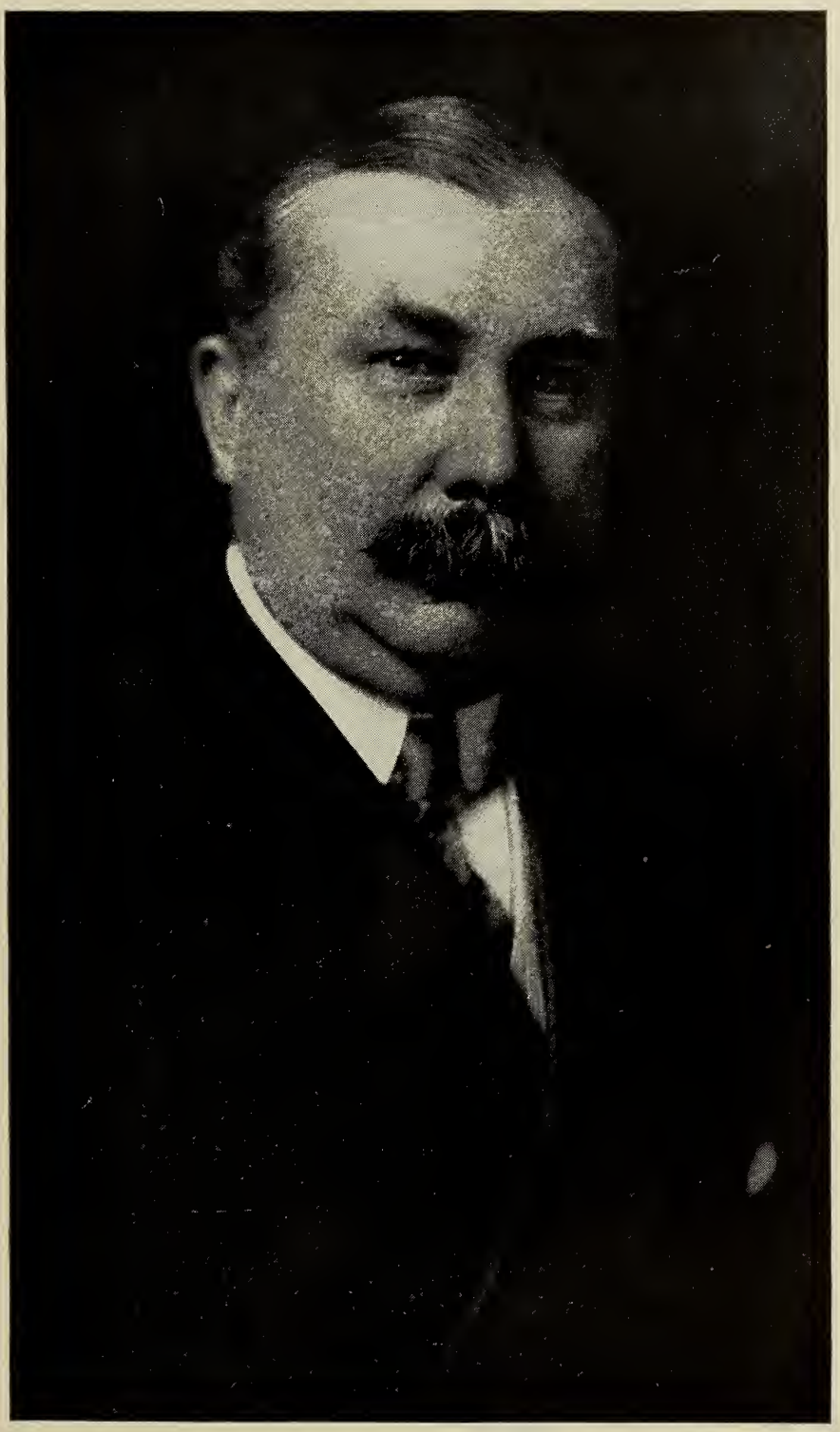

LEWIS LINN McARTHUR
PRESIDENT 1912

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TWENTY-SECOND ANNUAL MEETING

TRANSACTIONS OF THE

WESTERN SURGICAL ASSOCIATION

Twenty-Second Annual Meeting

Cincinnati, Ohio, December 20 and 21, 1912

MINNEAPOLIS
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OFFICERS AND EXECUTIVE COUNCIL, 1913

PRESIDENT

Jabez N. Jackson, M. D. - - Kansas City

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J. Rilus Eastman, M. D. - - Indianapolis

SECOND VICE-PRESIDENT

Major J. Seelig, M. D. - - - St. Louis

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Arthur T. Mann, M. D. - - Minneapolis

EXECUTIVE COUNCIL

Lewis L. McArthur, M. D., Chairman - Chicago
Amos W. Abbott, M. D. - - Minneapolis
William W. Grant, M. D. - - - Denver
Malcolm L. Harris, M. D. - - Chicago
John P. Lord, M. D. - - - - Omaha

CHAIRMAN COMMITTEE OF ARRANGEMENTS

Willard Bartlett, M. D. - - St. Louis

EX-PRESIDENTS AND MEETING PLACES

PRESIDENT	PLACE	YEAR
S. S. Todd*	Topeka	1891
Milo B. Ward*	Kansas City	1892
Milo B. Ward*	Des Moines	1893
Lewis Schooler	Omaha	1894
John E. Summers, Jr.	Kansas City	1895
Thomas J. Beattie	Topeka	1896
Joseph Eastman*	Denver	1897
David S. Fairchild	Omaha	1898
Homer C. Crowell	Des Moines	1899
O. Beverly Campbell	Minneapolis	1900
August F. Jonas	Chicago	1901
James E. Moore	St. Joseph	1902
Alex. Hugh Ferguson	Denver	1903
Charles H. Mayo	Milwaukee	1904
Harvey D. Niles*	Kansas City	1905
Malcolm L. Harris	Salt Lake City	1906
Charles W. Oviatt*	St. Louis	1907
William W. Grant	Minneapolis	1908
Arthur L. Wright	Omaha	1909
John P. Lord	Chicago	1910
Amos W. Abbott	Kansas City	1911
Louis W. McArthur	Cincinnati	1912

*Deceased.

The Western Surgical Association does not hold itself responsible for, or necessarily endorse, any of the papers printed herein.

ARTHUR T. MANN, Secretary-Treasurer.

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Executive Session

December 20, 1912—First Day—Morning Session

The Association met at the Sinton Hotel, Cincinnati, Ohio, and was called to order at 9:40 A. M., by the President, Dr. Lewis L. McArthur, of Chicago, who introduced Dr. Willard D. Haines, Chairman of the Local Committee of Arrangements.

Mr. Simeon M. Johnson, Vice-Mayor of Cincinnati, in the absence of the Mayor, delivered an address of welcome on behalf of the City, Dr. D. W. Haines, Cincinnati, spoke for the local medical society, and Dr. C. A. L. Reed gave the Association a welcome on behalf of the Cincinnati Academy of Medicine.

President McArthur: I want to ask the assistance and the cordial help of the Association in having their representative carry out their desires in this meeting. The hour is now past, and as is customary, we start with the scientific program at once. As the first three papers on the program deal with allied topics, if it meets with your approval, the discussion of the three papers will occur at one time after the completion of the reading of them. What is your pleasure?

On motion, the suggestion of the Chair was adopted.

Dr. F. Gregory Connell, of Oshkosh, Wisconsin, read a paper entitled "Etiology of the Lane Kink, Jackson Membrane, and Cecum Mobile."

Dr. J. R. Eastman, of Indianapolis, Indiana, followed with a paper entitled "Relation of Jonnesco's Parietocolic Fold and Other Fetal Folds to Jackson's Membrane and Similar Peritoneal Anomalies."

Dr. A. E. Benjamin, of Minneapolis, Minnesota, read a paper entitled "The Co-relation of Appendicities, Mucous Colitis, and Membranous Pericolitis."

These three papers were discussed together by Drs. Knott, Haines, Jackson, Grant, Eisendrath, and Dennis, the discussion being closed by Drs. Connell and Eastman.

Dr. Jabez N. Jackson, of Kansas City, Missouri, read a paper entitled "Retrocecal Appendicitis," which was discussed by Drs. Grant, Dorsett, and Reynolds.

Dr. R. C. Coffey, of Portland, Oregon, read a paper entitled "The Application of a Protected Gauze Pack Used as a Quarantine in Certain Abdominal Operations."

This paper was discussed by Drs. Wetherill, Jepson, Knott, Seelig, Mayo, and, in closing, by the author of the paper.

Dr. Maurice Kahn, Leadville, Colorado, read a paper on "Gun-shot Wounds of the Abdomen," which was discussed by Dr. Grant.

On motion, the Association adjourned until 2 P. M.

First Day—Afternoon Session

The Association reassembled at 2 P. M., and was called to order by the First Vice-President, Dr. Walter B. Dorsett.

Dr. Clifford U. Collins, of Peoria, Illinois, read a paper entitled "Two Cases of Obstruction of the Bowels from Unusual Causes."

The paper was discussed by Dr. Black, and, in closing, by the essayist.

Dr. S. C. Beede, of David City, Nebraska, read a paper entitled "Operative Technic in Intussusception," which was discussed by Drs. Eisendrath, Collins, Haggard, and, in closing, by the author.

Dr. L. L. McArthur, of Chicago, delivered his address as President. He selected for his subject "Transplantation of Tissue."

Dr. Charles H. Mayo, of Rochester, Minnesota, read a paper entitled "Observations on German Surgical Clinics."

Dr. M. G. Seelig, of St. Louis, Missouri, read a paper entitled "An Experimental Study of the Sodium Salts in Shock," which was discussed by Drs. Wetherill, Fisher, and, in closing, by the author of the paper.

Dr. James F. Percy, of Galesburg, Illinois, read a paper entitled "Osteitis Fibrosa Cysticus: Report of a Case, with Lantern Demonstration," which was discussed by Drs. Lewis, Smith, and McArthur.

Dr. B. B. Davis, of Omaha, Nebraska, read a paper on "How Much Pathology Can the Gall-bladder Have and Still Functionate?"

The paper was discussed by Dr. C. H. Mayo.

Dr. Daniel N. Eisendrath, of Chicago, read a paper entitled "Pyelotomy for the Removal of Renal Calculi," which was discussed by Drs. Jepson and Abbott.

Dr. William Hessert, of Chicago, read a paper on "Observations on the Radical Cure of Inguinal Hernia."

This paper was discussed by Drs. Nicholson, Ben-

jamin, Connell, Harris, Eisendrath, and, in closing, by the essayist.

On motion, the Association adjourned until 9 A. M., Saturday.

DECEMBER 21, 1912—SECOND DAY—MORNING SESSION

The Association met at 9 A. M., and was called to order by the President.

Dr. J. W. Andrews, of Mankato, Minnesota, read a paper entitled "Spinal Anesthesia in Surgery."

This paper was discussed by Drs. Moore, Harris, Abbott, Beede, McArthur, and, in closing, by the author of the paper.

Dr. H. G. Wetherill, of Denver, Colorado, read a paper entitled "The Growth, the Death, and the Regeneration of Bone," which was discussed by Drs. Moore, Eisendrath, Mayo, Mann, Kanavel, Porter, Quain, Harris, Lemon, Smith, McArthur, and, in closing, by the essayist.

Dr. D. W. Basham, of Wichita, Kansas, read a paper entitled "Ankylosis of Both Temporomaxillary Articulations," which was discussed by Dr. McArthur.

Dr. John E. Summers, of Omaha, Nebraska, read a paper entitled "Boning of the Thoracic Precardial Wall in Certain Affections of the Heart; Report of a Case."

This paper was discussed by Dr. Moore and, in closing, by the author of the paper.

Secretary Mann presented the report of the Executive Council as follows:

Report of the Executive Council

There were present at the meetings of the Executive Council, Dr. Abbott, Chairman, and Drs. McArthur, Lord, Wright, Moore, Grant, Harris, and Mann.

The following are recommended as officers for the coming year:

President—Jabez N. Jackson, Kansas City.
First Vice-President—J. Rilus Eastman, Indianapolis.
Second Vice-President—Major G. Seelig, St. Louis.
Secretary and Treasurer—Arthur T. Mann, Minneapolis.

Executive Council:

William W. Grant.
Malcolm L. Harris.

The following are recommended for membership:

James P. Aylen, Fargo
Edmund A. Babler, St. Louis
Emil G. Beck, Chicago
John F. Binnie, Kansas City
Arthur N. Collins, Duluth
J. Frank Corbett, Minneapolis
Carl B. Davis, Chicago
Elsworth Eliot, as honorary member, New York City
Oliver J. Fay, Des Moines
William Fuller, Chicago
Charles D. Lockwood, Pasadena
Robert A. Hanna, Peoria
William T. Reynolds, Kansas City
Charles N. Smith, Toledo
Roger T. Vaughan, Chicago

The Treasurer's report was approved and is as follows:

The Treasurer's Report

RECEIPTS

Balance on hand December, 1911	$623.97	
Fees and dues to December, 1912	1,335.00	
Extra banquet tickets for guests	65.00	
Total		$2,023.97

DISBURSEMENTS

Paid for exchange on checks and drafts	2.65	
Paid for printing	68.50	
Paid William Whitford for reporting the Kansas City meeting	164.50	
Paid for 250 vols. Transactions	750.40	
Paid for honorarium to Secretary	100.00	
Paid for postage, express, etc	21.55	
Paid for banquet at Cincinnati	350.00	
Paid for badges for this meeting	25.00	
Total		$1,482.60
Balance on hand		$541.37

As a place of meeting for 1913, the Committee recommended St. Louis, on Friday and Saturday of the week immediately preceding Christmas, with Willard Bartlett as Chairman of the Committee of Arrangements.

The following resolutions are recommended for adoption:

Resolved, That it is the intention of this Association not to countenance the practice of fee-splitting in its members nor in its applicants for membership, and that we incorporate in the application-blank for membership in this Association a clause to be signed by the applicant stating that he does not now practice, and will abstain in the future from the practice of, fee-splitting in any form, and that he will not countenance it in others.

It is further suggested that this Association would be pleased to receive the resignation of any member, if such there be, who feels that he is not willing to live up to this provision.

Resolved, That the Western Surgical Association ask the co-operation of the various American surgical associations and the Regents of the State Universities to co-operate with this

Association in the suppression of both the secret and the open fee-splitting evil in its various forms.

The President: What will you do with the report of the Executive Council?

Dr. S. C. Beede: I move its adoption as read.

Dr. Walter B. Dorsett: I second the motion.

Dr. Miles F. Porter: Does the recommendation say "secret fee-splitting"?

The President: It does.

Dr. Porter: While I am very much in favor of that part of the report, I would like to see it changed so that open fee-splitting would also be relegated to the background. There are some men who would openly split a fee, which is not quite as reprehensible as secret fee-splitting, because it takes a braver man to do it in the open than under cover; but it should be condemned along with secret fee-splitting, and if the committee will cut out that word, I think it would be a good thing.

Secretary Mann: The motion as passed in the Executive Council did not have the word "secret" in it; so it is understood that the word secret should be omitted.

Dr. J. W. Andrews: I think that point is well taken, and I would therefore move that the word secret be stricken out.

Motion seconded.

Dr. Dorsett: I hope the motion will not prevail. The very fact that there is secret fee-splitting makes it offensive. It seems to me the word "secret" is the most essential part of the whole thing. If it is open and above board, it is not so objectionable, bad as it is, but when it is secret, that is where the harm

comes in. I move to amend that the words "open and secret" be inserted in the report.

Motion seconded.

Dr. Porter: As I understand the motion, fee-splitting in any form is condemned. My idea is to get rid of the word "secret," as there are some men who are still splitting fees.

Dr. Andrews: I suppose what is meant by the word "secret" is that the patient or the one paying the fee is not to know anything about it.

Dr. Dorsett: Yes.

Dr. Andrews: I favor that both open and secret fee-splitting be condemned by this Association.

The original motion to adopt the report, as amended, was put and carried unanimously.

Dr. M. L. Harris: A short time ago this Association was unfortunate in losing by death one of its ex-presidents, a man with high attainments, greatly respected, and deeply beloved by all who knew him. I move that the members of this body rise and bow their heads in silence for a minute in memory of the departed, Dr. C. W. Oviatt, of Oshkosh, Wisconsin.

Motion seconded and carried.

Dr. Daniel N. Eisendrath: In connection with our symposium yesterday on the subject of movable cecum, there arose a discussion as to the presence or absence or significance of the Jackson membrane, and at that time in my remarks I suggested that it would be a good thing for the members of this Association to study this question. Accordingly, I wish to make a motion that a committee of three be appointed by the President to undertake a collective investigation, during the coming year, of the operations and results obtained by the members of the Association of lesions

of the large and small intestine with relation to movable cecum, Jonnesco's fold, and similar conditions, and that this committee report at the December meeting in 1913.

Motion seconded and carried.

Drs. D. N. Eisendrath, J. R. Eastman, and Howard Hill were appointed as this committee.

Dr. Joseph F. Smith, Wausau, Wisconsin, read a paper entitled "Fracture of the Carpal Scaphoid."

Dr. Carl E. Black, of Jacksonville, Illinois, read a paper entitled "A Method of Bone-Plating."

Dr. W. D. Haines, of Cincinnati, Ohio, read a paper on "Some Phases in the Modern Treatment of Fractures."

These three papers were discussed together by Drs. Hessert, Percy, Fairchild, Plummer, Lord, Ransohoff, and, in closing, by Dr. Haines.

The President-elect, Dr. Jackson, was introduced and said:

The opportunity is certainly inopportune for me to make a speech. I will simply say that I appreciate very highly this distinguished honor, one of the highest honors any man can aspire to and, recognizing this organization as a working body, and not a talking organization, I am sure a speech at this time is out of place, so I simply thank you. I shall endeavor to the best of my ability to make the next meeting equal to those in the past. (Applause.)

On motion the Association adjourned until 2 P. M.

Second Day—Afternoon Session

The Association reassembled at 2 P. M., and was called to order by the Vice-President, Dr. Ricketts.

Dr. Reuben Peterson, of Ann Arbor, Michigan, read a paper entitled "The Present Status of the Radical Abdominal Operation for Cancer of the Uterus," which was discussed by Drs. Dorsett, Percy, Haines, Jepson, and, in closing, by Dr. Peterson.

Dr. Walter B. Dorsett moved that the President appoint a committee of five to take steps towards formulating a plan to fight cancer.

The motion was seconded and carried.

Drs. Reuben Peterson, W. B. Dorsett, H. G. Wetherell, R. C. Coffee, and A. W. Abbott were appointed as this committee.

Dr. E. P. Quain, of Bismarck, N. D., read a paper entitled "Pregnancy in a Rudimentary Cornu of Uterus Unicornis, and Report of a Case with Full Term Fetus."

Dr. A. A. Law, of Minneapolis, Minnesota, read a paper on "Ventral Tumors of the Sacrum," which was discussed by Drs. Judd, Jackson, and, in closing, by Dr. Law.

Dr. Miles F. Porter, of Fort Wayne, Indiana, read a paper entitled "Co-existing Infection and Sarcoma of the Thyroid."

This paper was discussed by Drs. Mayo and McArthur.

Dr. Allen B. Kanavel, of Chicago, read a paper on "The Late Results of Operation upon the Hypophysis, with a Report Upon the Post-Operative History of a Patient with the Froelich Type of Disease."

This paper was discussed by Drs. Mayo, McArthur, and, in closing, by the essayist.

Dr. W. A. Dennis, of St. Paul, Minnesota, read

a paper entitled "Bilateral Cerebral Abscess Involving the Motor Areas."

This paper was discussed by Dr. Moore.

The President: Before the Association adjourns, I desire to express my intense appreciation of the honor which they have conferred upon me in making me the President for the past year. What good there has come from the program this year is due entirely to the efforts of our Secretary, Dr. Mann, and not to the President, and he should receive the credit. He should remain always the permanent Secretary.

I desire to say also, gentlemen, that my respect for, esteem of, and pride in, this Association has been very decidedly increased since my participation in the proceedings of its Executive Council. It is the only Association with which I have had intimate connection, in which there is absolute absence of any political pull or trial to gain office. Even as the junior member of the Executive Council, when I asked innocently if a certain man had ever held office in this Association, the reply was, "No; let us make him an officer," and every one of the committee joined in the proposition of voting for names that were suggested as officers, and I think we may all be proud to belong to such an Association.

I thank you. (Applause.)

Dr. Charles H. Mayo: I move that a rising vote of thanks be extended to the Cincinnati Academy of Medicine, to the Local Committee of Arrangements, to the Commercial Club, and the Sinton Hotel for the efforts made to make our stay in this city as pleasant as possible. The motion was seconded and unanimously carried.

As there was no further business, either scientific or otherwise, to come before the meeting, on motion the Association adjourned to meet in St. Louis, Missouri, in December, 1913.

President's Address

THE TRANSPLANTATION OF TISSUE

LOUIS L. MCARTHUR, M. D.

CHICAGO, ILLINOIS

Fellows of the Western Surgical Association:

Let me depart from the practice usual on similar occasions and, instead of reviewing the work and progress of the dying year, devote the allotted time to an individual topic, which to me, at least, appears a live and growing one. I refer to the transplantation of tissue or tissues, simple or complex, more or less living; lifted bodily to another part without the pedicle, heretofore deemed absolutely essential to the vitality of the transplant or to the success of the procedure. Improvement in aseptic technic, combined with the better understanding of the essentials of growth, have now made possible what but a few years ago would have been deemed foolish to attempt.

To Reverdin and Thiersch belongs the credit, in my opinion, of first practically demonstrating the possibilities of growth of tissues so transplanted, possibilities that have culminated in the present remarkable results achieved in the Yale and Rockefeller Laboratories, "the growth of cells in vitro." This has been accomplished by planting in a plasma the cells to be grown, said plasma having been derived from homologous living sources, and maintained under conditions approximating, as closely as possible, those of

life. In this way Dr. Harrison (Henderson's laboratory) at Yale and Carrel at the Rockefeller Institute for Research have duplicated under the microscope what Reverdin and Thiersch did clinically in their skin-grafting. When done by the best technic, Thiersch skin-grafting simply resolves itself into the preparation of a surface secreting the plasma essential to the life and growth of the transplant, living cells conveniently held together by the epidermis. In a similar manner, Reverdin, by burying minute particles of skin deep in graulation-tissue, surrounded them with their essential pabulum, the plasma. Under these conditions growth frequently ensued. From such simpler pioneer experiences we have slowly learned the lesson, that in most instances not only a *homologous,* but an *autogenous,* plasma is desirable. Witness the greater frequency of successes attending the use of skin-grafts from the patient's own body, and then from those derived even from his immediate kin.

Chief among the factors militating against success here are ineffective organisms, whether saprophytes or pathogenic, since they digest and destroy both the essential plasma and the transplant. As above stated, it has now been possible to demonstrate on the stage of the microscope that cells supplied with sufficient appropriate plasma under conditions approximating those of life, will live and multiply, each retaining its particular characteristics. This extends even to their systematic grouping, renal cells following renal arrangement, thyroid, those of their sources, and heart-muscle, fibrils, even to keeping up their rhythmic beat. Given, then, the appropriate conditions any tissue may be transplanted, the difficulties increasing with the *vascularity,* rather than with the *complexity,* of structure, those structures requiring the least circulation proving the more successfully grafted.

Turning from the general to the particular, let me

instance fat, a tissue made up of a delicate living reticulum, stored with an enormously disproportionate amount of fatty globules. For its existence and maintenance it requires little else than plasma and asepsis under body conditions. Hence it can be transplanted free and in almost any bulk, provided these conditions be maintained.

For this reason, we may now look to see the cavities in bones filled, instead of with Mosetig-Moorhoff's plug or Beck's paste, by pads of living fatty tissue taken elsewhere from the patient, as was recently demonstrated at Garré's Clinic in Bonn; or, as the writer has several times done, by depositing such fatty tissue in areas heretofore treated by Gersuny's paraffin injections, it being particularly adapted to filling out those greater deformities not amenable to paraffin. Let me give as an example the cosmetic filling out of the deformity left after a partial resection of the lower jaw. This has given in the writer's hands most gratifying results. Encouraged by the successful transplanting of this tissue, until recently considered very liable to slough, the writer has stolen elsewhere from the patient an envelope of fatty fascia to protect the musculospiral nerve, just released from the callus of a humeral fracture, or a nerve-trunk just sutured after resection for neurofibroma. Again, the hygroma that develops after Murphy's interposition of fatty fascia, need no longer be obtained from a pedunculated flap taken from the immediate neighborhood of the joint, often at the sacrifice of structures essential to a good result and adding a complication to the operation, but may be secured elsewhere. Indeed, for the ankylotic hip or knee, the operation can be thus remarkably simplified.

While speaking of hygromata intentionally produced, special mention must be made of a graft now very generally used abroad, taken from the thigh, of

appropriate size. It is composed of a mass of subcutaneous fat with fascia lata. With its use adhesions between the brain-tissue and skull-wall are obviated. The fragment being inverted, the fascia lata takes the place of the missing dura, to the remaining edges of which it is sutured, while its fatty surface, lying in contact with the brain, should adhesions form, gives such a yielding attachment as to constitute a hygroma or cushion that lessens the tendency to a Jacksonian epilepsy. The first time one sees the relatively enormous masses used to fill the space left vacant by the removal of a brain tumor, or the dissection out of dura and motor area involved in focal epilepsy, the vision of a necrotic slough in a most undesirable position obtrudes itself, but with repetition of cases and the testimony of many continental surgeons, one finally realizes that under aseptic conditions almost any autogenous graft may remain viable in any other part of the donor's anatomy.

Tendons.—The fellows of this Association will recall the experimental work done twelve years or more ago by Dr. John Hollister and the writer, in which it was demonstrated that strips of tendinous fascia cut from the sheaths of the rectus of the dog, not only could be thus transplanted, but at the same time would withstand the trauma incident to being used as a living suture. At that time there were submitted microscopic slides of sections made through such tendons, both from the lower animals and from man obtained months and years afterwards, in which their preservation was demonstrable. In the past much chagrin has attended the most perfect aseptic implantation of tendons because of the adhesions, so prone to form between the implant and the tissues with which it was in contact. Recently, Dean Lewis has shown that if a fatty fascial graft be placed above and below such tendon or tendons, motion can be maintained. Similarly, tendons

caught in the cicatricial tissue after a burn or an infection, can again be made mobile if surrounded by a fatty envelope.

Let me again reiterate two suggestions, once before submitted to this Association: first, that there are two or three places in the body whence excellent material for such artificial tendon transplants can be obtained, either from the aponeurosis of the external oblique, as has been demonstrated in the autoplastic hernial work, or from the fascia lata of the thigh. From the fascia lata long, slender and strong tendinous strips suitable for the replacement of missing structures can easily be obtained. The second suggestion is to make the taking of the tendinous strip a two-step operation; the first of which should be the loosening and raising of the desired fragment from its normal position, then allowing it to become healed in, buried in the neighboring subcutaneous fat for a fortnight or more. After the lapse of sufficient time the strip of tendon should be dissected out with a covering of fatty tissue adherent to it, thus obviating the introduction of separate fatty tissue after Lewis' method.

As illustrating the early realization that certain tissues simply required a sufficient amount of plasma to maintain their vitality, let me recall the suggestion made, and shown practicable, by a Scottish dentist forty years ago. The grafting of freshly drawn human teeth in the comb of the cockerel, to be nourished there until such time as needed for re-implantation in the next appropriate case. In a similar way before the era of the recommendation by Hartley and Krause of an osteoplastic flap, the reader early began the conservation of the buttons removed by the trephine, in warm normal sterile salt solution, until the end of the operation, when their replacement invariably resulted, in aseptic cases, in their healing in by bony union. At that time, to obviate the tedious removal of several

buttons in order to uncover the necessary wide area, he had the instrument-maker construct a trephine of two and one-half inches diameter, and had the satisfaction of learning that discs of such dimensions would likewise heal in with bony union. Indeed, Dr. J. Frank has a specimen obtained years afterwards from a skull in which such a button is shown perfectly healed in over a psammoma hidden in the brain-tissue immediately beneath it, which failed of discovery at the time of operation and later resulted in death during a focal epileptic seizure. From these experiences in the cranial vault, I believe the deduction justified that the temporary subperiosteal resection of any sized or shaped portion of the flat bones will be followed by proper healing in of the fragment when reposed between periosteal layers. Imbued with the truth of this deduction, success has followed the temporary subperiosteal removal of a large frontal fragment, together with the supra-orbital arch and supra-orbital plate, while seeking access to the pituitary neighborhood. Its reposition has been accomplished without an untoward symptom and with perfect cosmetic result.

These, and the many experiences of others, demonstrate that bone formerly believed to require an actual blood-supply to maintain its existence, does live without it, and, healing in, serves the purpose for which it has been inserted. Whether the teachings of Murphy are true, that its cut surfaces must lie in contact with living osteogenetic elements to live or that it is slowly replaced by new bone, we do know that its use in this is justified by the results; hence, the applicability is infinite. The latest practical adaptation of this knowledge by Albee, in the artificial strengthening of the vertebral column weakened by Pott's disease, by the insertion of fragments of the tibia between the spinous processes of the patient, has been

the wonder and admiration of both general and orthopedic surgeons.

A fitting climax, demonstrating the extent to which this realization of the fact that not only simple but most complex structures can be so grafted, was shown the members of the Clinical Surgical Society by Lexer of Jena. I refer to the implantation of the human knee taken from one individual, and inserted between the sawn ends of femur and tibia in the patient just operated upon for a bony ankylosis of that joint. These patients could walk without crutches, showed a good degree of motion, and, while yielding no better results than American methods have given, illustrate the potentialities, not yet exhausted, of free transplantation. Unfortunately, these heteroplastic homologous grafts do not have the persistency of the autogenous transplant.

I do not propose to weary you with that other yet unwritten chapter, which, beginning with the successful implantation of "cold-storage" blood-vessels, bids fair to lead to triumphs in surgery that to have hinted at a few decades ago would have condemned the one suggesting it as of unsound mind.

Suffice it to say if some of you, encouraged by the data just reviewed, shall find in your future work a stimulus to search for still better methods and newer adaptations, far-reaching benefits will accrue to humanity, to whom our best efforts are ever devoted.

ETIOLOGY OF THE LANE KINK, JACKSON MEMBRANE, AND CECUM MOBILE

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OSHKOSH, WISCONSIN

The general acceptance of the above enumerated conditions as clinical entities, has led to much speculation and theorizing upon the etiological factors that may produce such results. These explanations may be divided, roughly, into inflammatory and non-inflammatory, and differentiation between them may at times be difficult, because of the fact, which has not been sufficiently emphasized, that inflammatory reaction occurring either as a result or as an independent coincident, may leave resultant adhesions that might confuse the question of etiology.

I shall confine my remarks to a consideration of the non-inflammatory factors, which offer a rational explanation for typical examples and may be either acquired or congenital. The acquired is best exemplified by the theory of Mr. Lane[1] "that these bands are crystallization of lines of force in Nature's attempt to correct ptosis of the colon."

A combination of both the congenital and the acquired factors is presented by Martin[2], who calls attention to the long-continued traumatism of the comparatively fixed portion of the terminal ileum, by the over-riding of the abnormally movable colon and other superimposed viscera.

The congenital theory suggested by Mayo[3], that the cecum descends behind the posterior parietal peritoneum, instead of in front, has not met with general acceptance.

The following explanation, based upon the theory of imperfect development, I first presented before the Chicago Gynecological Society on February 23, 1912[4]. Since then observation of cases of this character has given considerable support to this theory, as have likewise the publications of J. M. Flint[5], and Gray and Anderson[6].

In order to understand the probable developmental faults leading to these conditions, it will be necessary to review the normal development. (Figs. 1, 2, 3, 4, and 5, from Huntington[7].) During this the ileocecal junction migrates from below upward and to the left to form the splenic flexure; crosses the abdomen to the hepatic flexure, where it rotates on its long axis; descends; and is fixed in its normal, i. e., usual, location in the right iliac fossa.

In the available literature this most complicated embryological maneuver is spoken of under the caption, "Rotation of the Cecum," and it is in a large measure due to this use of a single term to describe these various changes that there has arisen considerable confusion regarding this matter. On closer analysis one finds that this so-called rotation, is comprised of three definite and distinctly different elements, viz., migration, rotation, and fixation.

By *migration* will be meant the journey of the ileocecal junction up, across, and down the right side of the abdominal cavity. (Figs. 1, 2, 3, 4, and 5.)

The term *rotation* will be confined to the actual axial rotation of the ileocecal junction, through an angle of 180 degrees in a direction from above downward, backward, and forward. (Figs. 3 and 4.)

By *fixation* is meant the blending or fusion of the posterior layer of the visceral peritoneum of the colon and the primitive mesocolon with the prerenal primitive parietal peritoneum, by means of which the advantages gained by the migration and the rotation are retained, and the ileocecal junction is held in its normal and desirable location in the right iliac fossa. Figs. 6 and 7, before fixation; Figs. 8 and 9, after fixation.

We have here reviewed the usual, the natural or normal development of the ileocecal junction, and are now ready to consider its possible anomalies.

Anomalies of Migration.—Migration may be (a) incomplete or (b) delayed. (a) Incomplete. With arrest of migration in its transit across the abdomen there will be an absence of an ascending colon and an incomplete transverse colon, or this may occur at the hepatic flexure, either before or after its fixation. (Figs 3. and 4.)

Before fixation the ileocecal junction may fall back to the left iliac fossa. (Fig 10.) This condition is unusual, but will account for some of the authentic cases of left-sided appendicitis in which there is not a transposition of the viscera. After fixation the transverse colon will be complete, but there will be an absence of the ascending colon. Such cases are not uncommon. I have met with one such case (Fig. 11), and Mayo[8], Flint[9], and others report similar conditions.

The most common arrest of migration occurs at any point between the hepatic flexure and the so-called normal location in the right iliac fossa. (Figs. 4 and 5.)

(b) Delayed. The cecum may be in its usual location, descent having been complete; yet there may be

evidence in support of the supposition that such descent has been delayed. The evidence will consist of adventitious bands or membranes extending from the prerenal parietal peritoneum, downward and forward to the anterior surface of the colon, i. e., a pericolic membrane, being probably the expanded or stretched line of primary fusion which had taken place while the colon was still in an undescended or subhepatic position. Fig. 12 shows the formation of a membrane in such a manner, and is taken from the very instructive monograph of Flint[5]. This author attributes likewise the ileal kink to this same anomalous development, but on this latter point we have, as yet, been unable to arrive at the same conclusion.

Anomalies of Rotation may be (a) non-rotation; (b) excessive rotation; or (c) reversed rotation.

(a) Non-rotation is most common with incomplete migration before the development of the hepatic flexure. It may likewise, but less frequently, persist after the formation of the hepatic flexure and the ascending colon. In such instances the terminal ileum enters the cecum from behind, or from the lateral, the right, side, instead of from the median, or left, aspect of the cecum, and may lie in contact with parietal peritoneum and fuse in such relationship as to cause subsequent obstruction, i. e., the Lane kink.

Huntington and Kelly[10] each report cases of this type. Stewart[11] mentions two, and personally I have encountered this anomaly twice (Fig. 13), in both of which cases there was a Lane kink with cecum mobile.

(b) Excessive rotation, that is, rotation in a normal direction, but of more than 180 degrees, even as much as 270 degrees, results in the ileum entering the cecum from before backward, instead of from the left or median aspect, as is usual. Fusion between

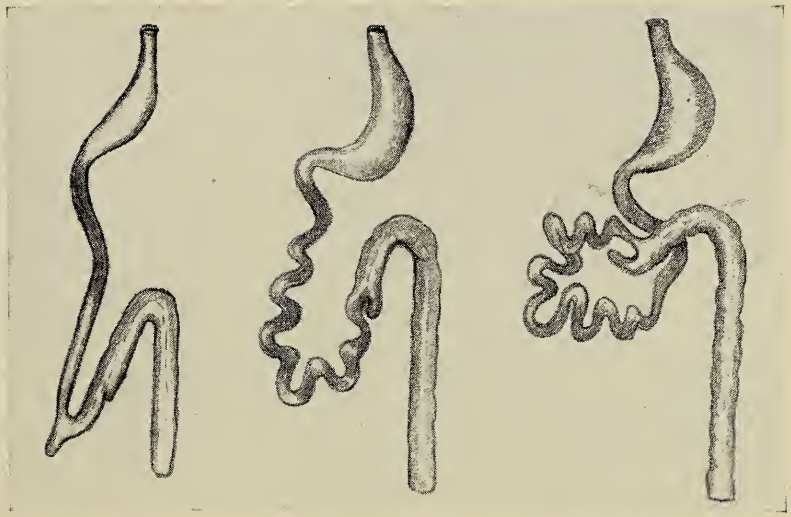


Fig. 1. Fig. 2. Fig. 3.

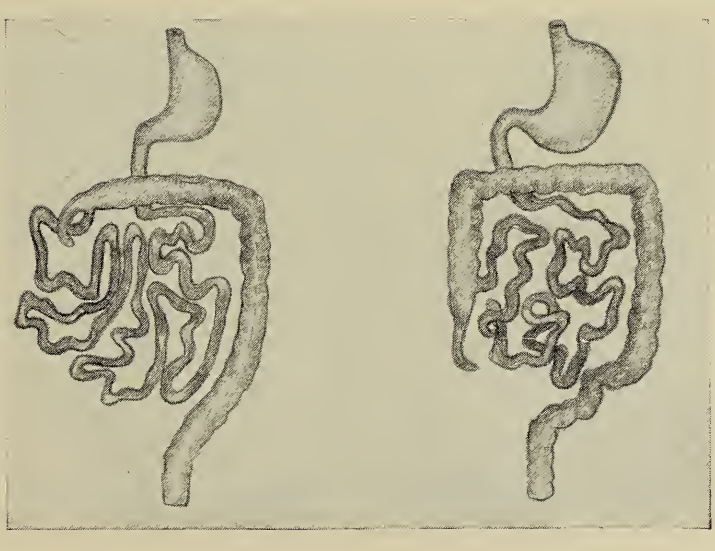
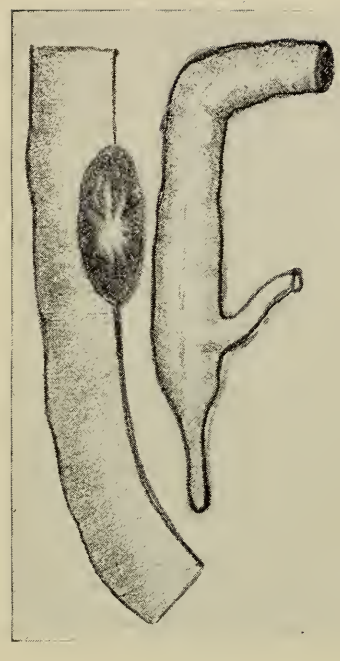


Fig 4. Fig. 5.

Migration and rotation of cecum, in normal development.



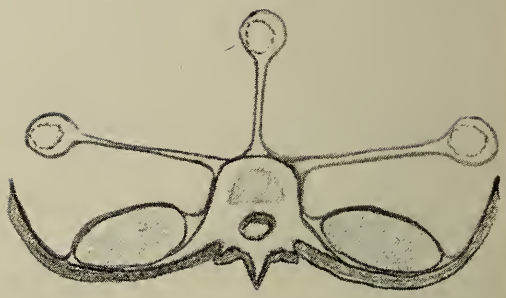
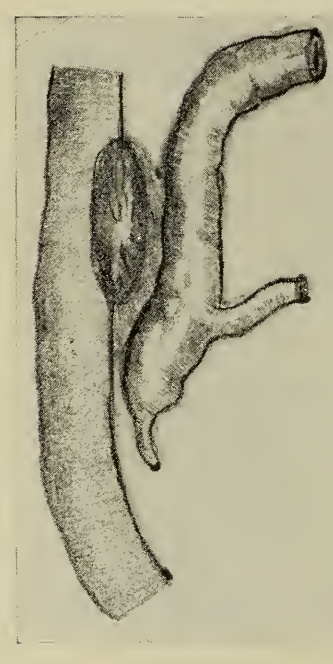


Fig. 6. Fig. 7.

Relations before fixation of ascending colon.



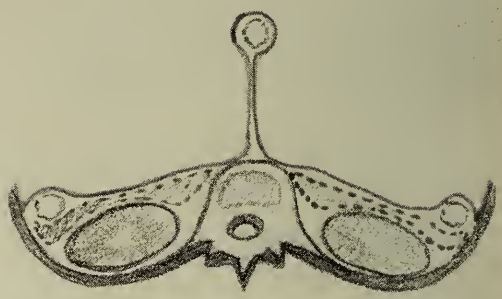


Fig. 8. Fig. 9.

Relations after fixation of the ascending colon.

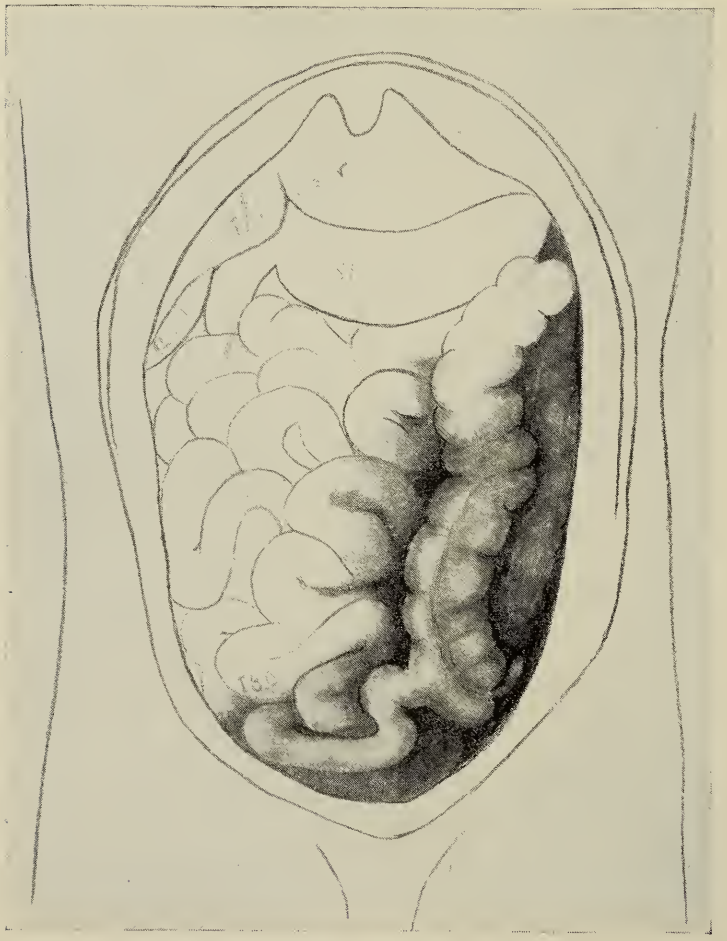


Fig. 10. Arrest of migration before rotation or development of hepatic flexure, with absence of transverse and ascending colon.

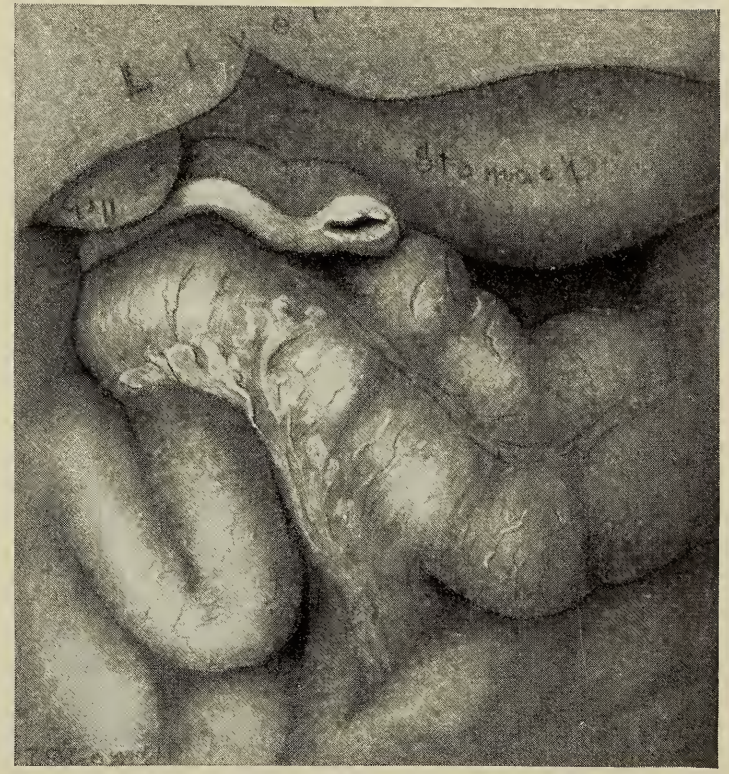


Fig. 11. Arrest of migration after rotation and development of hepatic flexure, with absence of ascending colon.

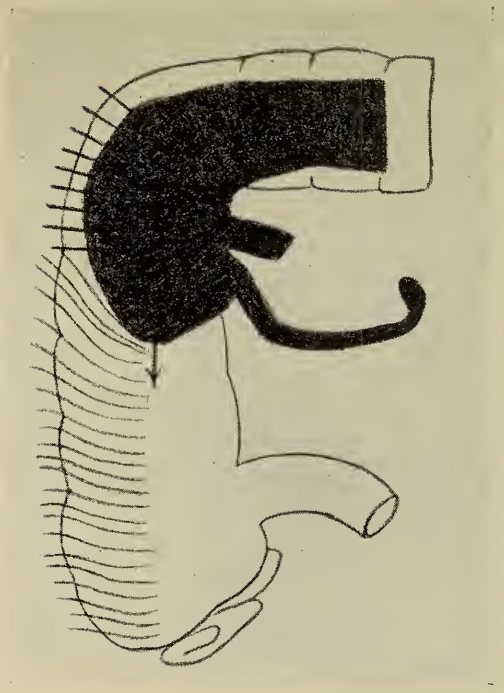


Fig. 12. Showing development of pericolic membrane. (Flint.)

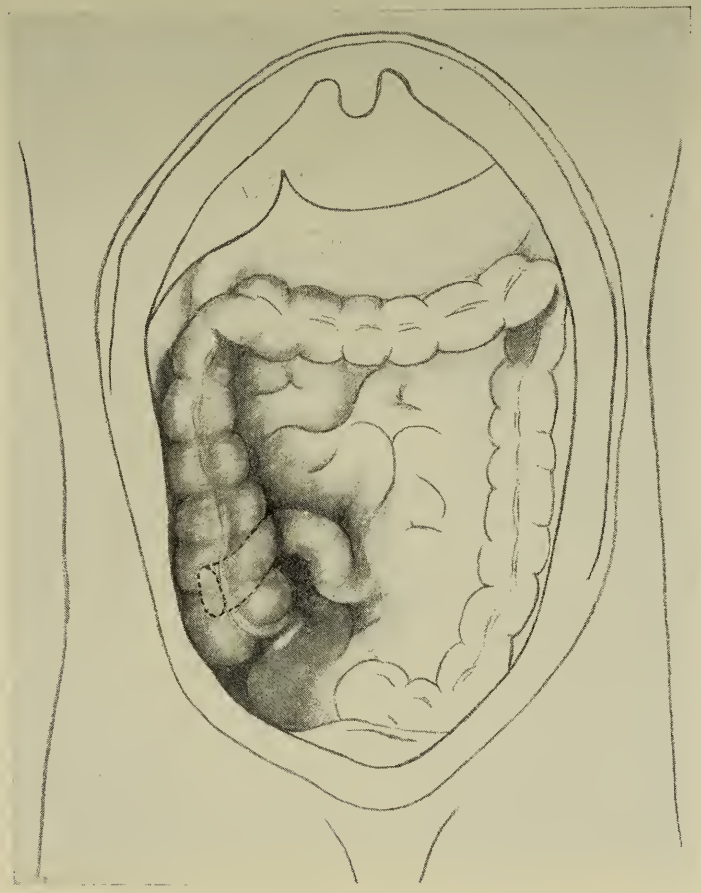


Fig. 13. Non-rotation with complete migration.

such apposed visceral and parietal peritoneal coats will result in fixation of the colon and terminal ileum in abnormal positions. (Fig. 14.)

Such a condition will probably persist until the bowel functionates; then, as the cecum and the terminal ileum contain contents which allow gravity to exert a pull, the tendency will be for the ileocecal junction to return from its exaggerated rotation and assume its normal relations. (Fig 15.) By so doing, the line of serous fusion previously formed will stretch and form a membrane or veil on the colon, that will accurately fulfill the detailed description of the pericolic or Jackson membrane, and the fixation of the terminal ileum that will simulate the Lane kink.

(c) Reversed rotation, in which rotation takes place from above forward and downward, instead of from above backward and downward, would result in a kink at the hepatic flexure, and a kink in the terminal ileum and in the ileocecal junction, the appendix becoming entangled in the primary mesentery in such a manner as to readily suggest the Jackson membrane, the Lane kink, or structures that might be called "adhesions." (Fig. 16.)

That a reversed rotation might occasionally take place, seems quite plausible, for, as Huntington says (page 240), "The apex of the cecum is frequently embedded among these terminal coils of the ileum;" yet, in the examination of a large number of ileocecal junctions in which one or more of these conditions were present, I have been unable to find satisfactory evidence of any such reversed rotation having taken place.

Anomalies of Fixation.—(a) Absence, complete or partial; (b) early; (c) of improper surfaces.

(a) Complete absence of fusion between the pari-

etal and visceral peritoneum of the ascending colon and mesocolon, will result in the so-called cecum mobile. (Fig. 17.) This is quite common and might be accounted for by late rotation in which coils of ileum prevented the apposition of the surfaces that usually fuse.

With normal fusion of the upper third, and absence below, the mobility will be less marked. If present in the upper two-thirds and lacking in only the lower third, the result will be approximately normal. Fusion of the lower portion of the colon, with absence at the upper part, may result in an angulation, and in marked cases will show the characteristic "double-barrelled colon," of which I have met with one typical example. (Fig. 18.)

In such cases adventitious ligaments, bands, or membranes are often seen, which may be belated, ineffectual attempts on the part of Nature to hold the viscera in its proper position, or may be due to secondary changes, due to the stasis caused by the angulation.

(b) Early fixation, before the complete descent of the ileocecal junction, has been considered in the discussion of delayed migration.

(c) Fixation of improper surfaces has been considered in the discussion of delayed migration and of normal and excessive rotation.

But even with normal migration and rotation the usual fusion, which ceases at the lower margin of the ascending mesocolon, may become excessive and, with either normal or defective fusion above this point, extend a greater or less length along the terminal ileum, causing a condition necessary for the development of the so-called Lane kink.

The theory of Gray and Anderson and of J. M. Flint, that the Jackson membrane is the right margin of the great omentum, which, during development

Fig. 14. Normal migration, with excessive rotation and fixation.

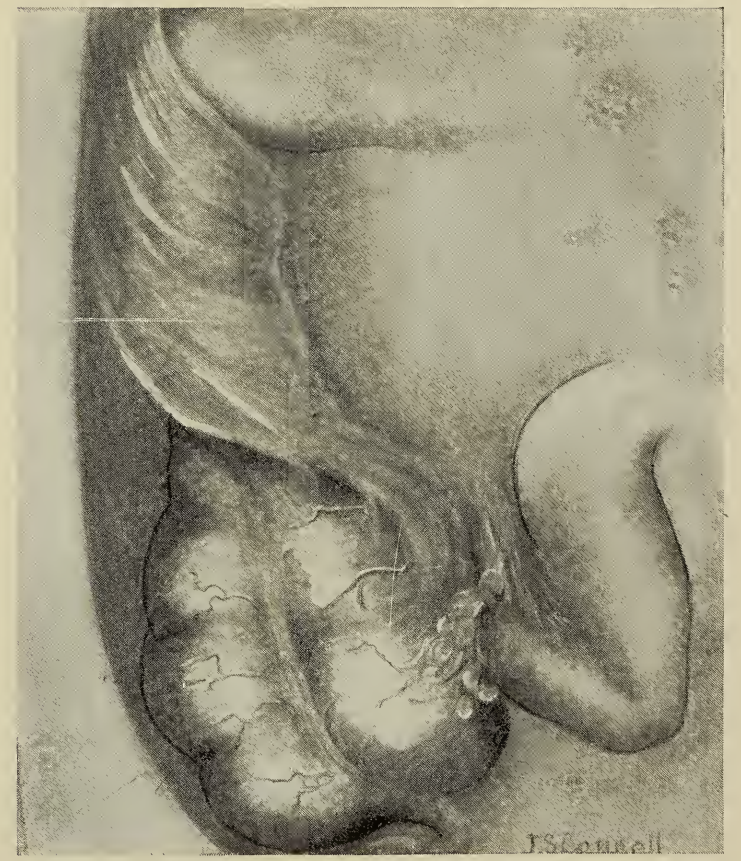


Fig. 15. Exaggerated rotation of Fig. 14, corrected by return to normal rotation.

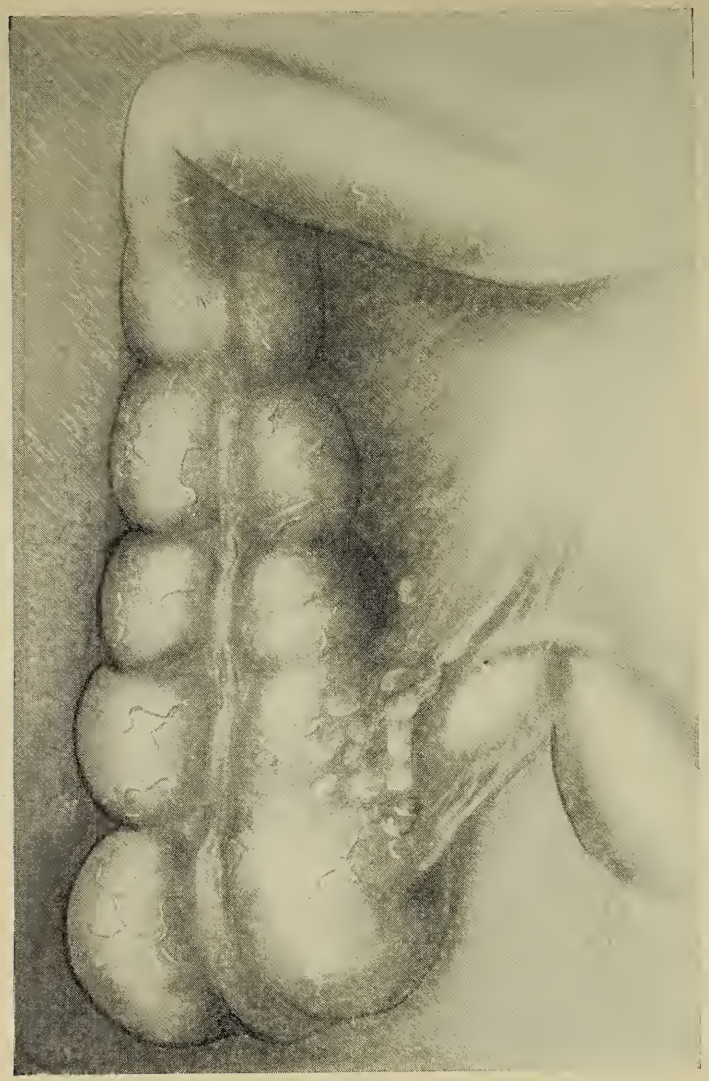


Fig. 16. Absence of fixation—"cecum mobile."

Fig. 17. Partial fixation—"double-barrel" colon.

becomes adherent to the dorsal parietal peritoneum opposite the flexure and along the outer side of the ascending colon for a varying distance, is another example of fusion of improper surfaces. Subsequent descent of the cecum may give to this portion of the omentum the characteristics of the pericolic membrane and seems to be a very rational explanation, and without doubt it is what takes place in certain of these cases.

CONCLUSIONS

1. Anomalies of development offer a rational explanation of these conditions.

2. Coincident or resultant inflammation, may cause confusion.

3. Describing the embryological changes in the ileocecal region under the single term "rotation" likewise causes confusion.

4. Such changes are migration, rotation, and fixation, one or more of which may be imperfect.

5. The Jackson, or pericolic, membrane may be due to excessive rotation, delayed migration, or early or anomalous fixation.

6. The Lane kink may be due to excessive or anomalous fixation.

7. The cecum mobile is due to an absence of fixation.

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FOR DISCUSSION SEE PAGE 71

THE FETAL PERITONEAL FOLDS OF JONNESCO, TREVES, AND REID, AND THEIR PROBABLE RELATIONSHIP TO JACKSON'S MEMBRANE AND LANE'S KINK

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It is the purpose of this paper to call attention to the striking similarity of the fetal peritoneal fold described by Jonnesco and Juvara ("Anatomie des ligaments de l'appendice vermiculaire et de la fossette ileo-appendiculaire," Le Progrés Medical, tome xix., p. 273) and designated by them "the parietocolic fold" to the adult peritoneal anomaly described by Jackson as membranous pericolitis and generally known as "Jackson's membrane."

Hofmeister gives credit to Virchow for the first description of this membrane as found in the adult; and Binnie, Lane, Hofmeister, Coffey, Connell, Crossen, Flint, C. H. Mayo, Pilcher, Gerster, F. H. Martin, and Hall have all contributed to the present knowledge of this matter by reports of cases, and many of them have submitted hypotheses to explain the origin of Jackson's membrane. Jackson's early articles were met with hesitating credulity on the part of many who, if they admitted the fact of the existence of these membranes, ascribed their presence to simple inflammation and adhesion-formation. Such a view could find very few supporters now.

It is the further purpose of this paper to call attention to the probable causal relationship between the bloodless fold described by Treves ("The Anatomy of the Intestinal Canal and Peritoneum in Man," Hunterian Lectures, 1885) and a pocket-like anomalous peritoneal reflection which is not rare in the adult, and which passes from the mural peritoneum upon the right side quite low down, extending upward and inward over the caput coli and vermiform appendix to be attached to the last two or three inches of the ileum, and to the peritoneum of the caput coli. It forms the boundary of a pre-colic fossa in which the cecal head and appendix may rest as in a pocket. It is likely that the caput coli with the appendix is, in not rare instances during operations for appendicitis, shelled out of this peritoneal pocket, the peritoneal fold, that is, the bloodless fold of Treves, which forms the pocket, being looked upon by the operator as an affair of adhesion-formation.

Moreover, although conceptions of Lane's ileopelvic band, the structure to which is ascribed an important part in the causation of Lane's kink, are somewhat varying, it may be well in discussing the nature and origin of this band to recall that Douglas Reid, of the University of Cambridge ("Studies of the Intestine and Peritoneum in the Human Fetus," Journal of Anatomy and Physiology, vol. xlv, p. 73), has described, under the name "genitomesenteric fold," a rather common fetal fold of peritoneum which passes from the terminal portion of the ileum into the pelvis. Concerning this genitomesenteric fold, which may be found readily in a surprisingly large percentage of fetuses after the seventh month, or even after birth at term, the question may be raised fairly as to whether it is not related to angulations or gravitations or other deformities of the terminal ileum to which it is attached.

The parietocolic fold of Jonnesco and Juvara arises, in most cases, from the peritoneum at the left or inner side of the ascending colon, passing over the anterior aspect of the ascending colon in an upward slanting direction. It is attached to the parietal peritoneum at the right of the ascending colon. It may adhere to the anterior and lateral aspects of the colon. Read (*ibid*) ascribes to this fetal fold practically the same relations as are presented by the parietocolic fold or Jackson's membrane when found in the adult.

By courtesy of Dr. H. R. Alburger, Professor of Pathology in the Indiana University School of Medicine, twenty-eight fetuses were examined in the course of the preparation of this paper. All of these had passed the sixth month, and varied in length from twelve to thirty-two centimeters. In the series of twenty-eight fetuses, five presented the parietocolic fold. Reid found it present in three out of a series of twenty fetuses, or in about 15 per cent of the fetuses examined. In a fetus twenty centimeters in length, I found a parietocolic fold which enveloped the ascending colon in such a manner as to constrict it to less than one-half the transverse diameter of the caput coli, which bulged forward, balloon-like, below the grasp of the encircling peritoneal fold. Observing that the blood-vessels of the parietocolic membrane continued over on the mural peritoneum, it was recalled that Hall, in his report of an examination of one of Dr. Jackson's first cases of membranous pericolitis in the adult, noted also that the blood-vessels of the parietal serosa were continuous with those of the anomalous membrane which enveloped the ascending colon.

In a fetus at term a parietocolic membrane was found which began at the inner side of the ascending colon and passed around to the front of the colon, where it branched, the larger leaf passing downward,

spreading, web-like, along the colon and spanning the concavity of an elbow-bend in the colon, the sharp free edge of the membrane being upon the inner, or left, side. It was suspected that the flexure of the colon referred to was due to the presence of the membrane, which passed from one limb to the other limb of the elbow. The bloodless fold of Treves was also present in this case.

In a fetus twenty-two centimeters in length, a web-like peritoneal covering was found upon the ascending colon, which passed over upon, and was continuous with, the serosa covering the great omentum. This membrane was closely adherent to the serous covering of the ascending colon, as if the layers had become fused together.

In a fetus seventeen centimeters in length, Reid found the posterior (direct) and the anterior (reflected) laminæ of the great omentum firmly adherent, and the posterior (direct) lamina fused completely to the colon. In the first case of so-called "membranous pericolitis" in the adult which I operated upon, the web-like anomalous peritoneal membrane fused with the serosa of the ascending colon, as described above. The veil-like membrane could not be removed without taking with it the normal peritoneal covering of the colon, thus leaving the latter raw and bleeding. In this case the web-like peritoneal membrane passed over on the gastrocolic omentum. Whether this was a case of actual fusion of the two peritoneal layers of the omentum with the peritoneal surface of the colon, was not proven. This was owing to the circumstance that the author was not aware at that time of the fact of the occasional existence of such a relationship. Flint (Johns Hopkins Bulletin, vol. xxiii, p. 260) states that "When the rotation of the gut is complete, the mesogastrium balloons for-

ward, and the formation of the great omentum begins. This fuses later with the transverse colon, and the gastrocolic omentum thus comes into existence. Usually this fusion takes place only along the transverse colon, establishing the usual relationships. Not infrequently, the omentum attachments extend further to the right, even to the abdominal parietes." Thus it will be seen how the two peritoneal layers of the great omentum, unseparated by fat, may come to lie upon the serosa of the ascending colon, as well as upon the transverse colon.

To return to the parietocolic peritoneal fold of Jonnesco and Juvara: This membrane, in the five cases observed, could readily be separated from the underlying colic peritoneum. This is true of many cases of Jackson's membrane, and this would hardly be the case were it a product of membranous pericolitis alone, as suggested by Jackson, or of mechanical irritation alone, as indicated by Martin. Concerning the origin of Jonnesco's fold: We have found several succeeding stages of its formation, which indicate that adhesions may form between the serosa of the cecum and the mural serosa while the cecum is still subhepatic. The subsequent descent, with torsion or rolling inward upon the long axis, draws the parietal serosa over in front of the ascending colon in a slanting or spiral direction, like a ribbon wound spirally around a rolling cylinder.

In twenty-eight fetuses examined, the bloodless fold of Treves was found nine times. It passed from the lateral posterior parietal peritoneum to the terminal part of the ileum and the caput coli, fusing with the peritoneum covering the intestine. It was always more or less fan-shaped, the broad attachment being to the mural peritoneum. The intestinal end was always the more narrow. It formed a pocket in which the vermiform appendix and more or less of the caput

coli and cecum were ensconced. This fold is described by Douglas Reid as follows: "Passing from the mesenteric border of the appendicular cecum, and from the non-mesenteric border of the last part of the ileum, to the posterior and lateral abdominal wall, is a sheet of the peritoneum. This forms the anterior wall of a fossa completely closed save below. Its orifice is bounded in front by the free lower edge of the peritoneal sheet; medially by the ileum and the adhesions which bind this down; and posteriorly and laterally by the parietal peritoneum."

The bloodless fold of Treves may adhere to the abdominal wall and resist for a time the completion of cecal torsion. It does not seem illogical to assume also, therefore, that the lower inner border of the bloodless fold of Treves may resist the drawing upward and inward of the last part of the ileum, thus preparing conditions favorable to the formation of Lane's kink, or actually causing it. Keeping in mind the attachments of the bloodless fold of Treves before cecal torsion or internal rolling upon the long axis has taken place, it is not difficult to understand how the cecum covers itself as it turns over with this sheet of peritoneum, just as a man rolls himself up in a blanket.

In Huntington's "Anatomy of the Peritoneum" (p. 241) will be found a description of cecal torsion. "When the cecum has come down from its hepatic position, the terminal coils of the ileum pass from right to left behind the cecum to enter the colon, and the concavity of the cecal pouch is directed upwards and to the right. After the final act of rotation has occurred, that is, after the cecal torsion has taken place, the ileum enters the large intestine from the left and from below, and the concave border of the cecum is directed to the left. This change in relative position

has been accomplished by a revolution of the colon and cecum through an arc of 180° around its own long axis, carrying the cecum above and behind the small intestine and bringing it into contact with the dorsal prerenal parietal peritoneum. At the same time the terminal coils of the ileum turn downwards and to the left. If this final step in the rotation of the large intestine fails to occur, with otherwise normal development of the parts, the ileum will persist in entering the large intestine from right to left after the cecum has obtained its final lodgment in the right iliac fossa."

Reid speaks of two cases in which the parietocolic fold is formed by a large appendix epiploica. The presence of the parietocolic fold and of the bloodless fold of Treves might be explained by assuming that before cecal torsion takes place a large appendix epiploica, or several appendices epiploicæ become adherent to the parietal peritoneum and elongate and spread out as the colon rolls over from right to left. It is difficult to understand how an adhesion between the colon and the mural peritoneum could occur except through an appendix epiploica, inasmuch as before cecal torsion is complete the colon and mural peritoneum are separated by coils of intestine, which lie in front of the colon below that part which is covered in front by the liver. At the right side lies the appendix, which, it should be noted, is proportionately very large in the fetus. It forms a loop at the side of the colon and cecum. The appendix, with the meso-appendix, completely separates the colon from the peritoneum at the right lateral abdominal wall. Reid remarks that at this stage of cecal torsion it appears to be impossible for any adhesion to form between the colon and the lateral abdominal wall, excepting through the medium of an appendix epiploica. Contemplation of an appendix epiploica in the adult

would certainly furnish an erroneous conception of the length and width of the fetal folds here considered. Reid's remarks concerning the possible rôle of appendices epiploicæ in the formation of these folds take the form of intimations only. He does not leave the impression that he would claim to have cleared up this somewhat obscure matter.

Whatever the origin of these folds in the fetus, there will be no doubt in the mind of any one who will examine the region of the cecum in a few fetuses that they exist before birth and are readily demonstrable in one form or other in approximately twenty per cent of fetuses after the sixth month. The fold of Treves was present in a larger percentage of cases.

If we admit that the bloodless fold of Treves is a reality, and that there is no error as to its being attached, as described, to the ileum and cecum, and if we admit that cecal torsion after such attachment is a fact, then there should be no surprise, in view of the frequency of the appearance of Treves' fold in the fetus, that occasionally we find the caput coli in a peritoneal pocket in the adult. I have shelled the caput coli and appendix out of this pocket in operations for so-called appendicitis several times. The caput coli does not fit loosely into the pocket. There seems usually to have been a more or less successful attempt at fusion of the bloodless fold of Treves with the caput coli and appendix, so that it is necessary to dissect out bluntly the appendix and cecal head, chiefly with a Mayo's blunt scissors.

Jackson's membrane is, as yet, indefinitely described. Its location is not constant. Without doubt membranes have been designated "Jackson's membrane" which Dr. Jackson would not have embraced under the name "membranous pericolitis." However, I know that some of my colleagues have looked upon as a

Jackson's membrane what was, in view of evidence now at hand, certainly a persisting bloodless fold of Treves, which held the caput coli and appendix as in a pocket of a shirt. I have found the bloodless fold of Treves to be present, not only in cases presenting, generally speaking, the symptoms of chronic appendicitis, but in two instances in the course of operations which did not concern the intestine at all. In both of these cases the last part of the ileum was drawn downward in a slight degree; but the persisting fetal fold was lax, and there had been no symptoms of Lane's kink.

The fold which was described by Douglas Reid and called by him "the genitomesenteric fold," passes from the mesentery of the last part of the ileum down into the pelvis, being attached in the fetus to the genital gland below. It will be seen, therefore, that it has a secondary connection with the ileum and through the peritoneum of the meso-appendix with the appendix itself, a connection which is, perhaps, responsible for the frequent association of appendicitis and oöphoritis. (Reid.) I have seen this fold in the adult. By lifting the last part of the ileum upward, a thin fold of peritoneum, which was quite loose, could readily be seen passing from the mesentery of the last part of the ileum over the brim of the pelvis to the region of the ovary. There was no sharp border of this fold upon the right side. It spread out on the right side into a rather narrow fold of Treves. Reid describes this genitomesenteric fold as passing under the appendix, whereas the fold of Treves passes over the caput coli and appendix. However, in our cases, the inner or left border of the bloodless fold of Treves has ended in each case below in the genital gland in the fetus, at the ovary in the female after birth, and at the internal abdominal ring in the male at term. Perhaps we have not seen the genitomesenteric fold as it was seen by

Reid. Certainly we have not seen it as a separate distinct fold, but rather as the inner prominent edge of the bloodless fold of Treves, passing from the terminal ileum to the genital gland. It is this genito-mesenteric fold of Reid, or the prominent inner border of the fold of Treves, as the case may be, which corresponds in its position and attachments to the ileo-pelvic band of Lane. The relationship between the fetal fold and Lane's band is, perhaps, only suppositional, but it seems not unlikely that they are identical.

Reid has further drawn attention to adhesions of the colon to the peritoneum of its own mesentery. I have seen in operations upon the adult the peritoneum of the mesentery of the ascending colon reflected upward and adherent to the colonic peritoneum for a distance of five inches. Byron Robinson noted this form of adhesion. ("Morphology of the Digestive Tract," *Journal of Anatomy and Physiology,* vol. xxxiii). I have observed it twice in the adult. It was unassociated with evidences of inflammation. As it was seen by Reid in the fetus, it is fair to suspect this reflection of the mesenteric peritoneum of being a persisting fetal adhesion. We have here strong evidence that Jonnesco's fold is due to adhesion with subsequent rolling inward or normal cecal torsion.

Concerning the relationship of the folds of Treves and Jonnesco to Jackson's membrane: It may be safely stated that not all anomalous membranes which have been designated as Jackson's membrane. are persisting fetal folds, as described above. The explanation which C. H. Mayo gives of the origin of Jackson's membrane, which is, in effect, that, after the peritoneum has formed, the rotating cecum coming down from its subhepatic position pushes ahead of it an extra covering of peritoneum, is about the only

hypothesis by which we can explain that form of Jackson's membrane which encloses the cecum and ascending colon as in a bag. Mayo's explanation has been corroborated by those who have made studies of the peritoneum in the fetus, the differences in conclusions being largely differences of language. Mayo spoke of "burrowing of the cecal head" under mural serosa as it descended and rolled inward, whereas adhesion formation while the cecum is subhepatic with subsequent descent and internal torsion seems best to describe our impressions.

However, fetal peritoneal reflections, folds, and adhesions are so numerous and varying that it requires no great stretch of imagination to think of the descending cecal head catching under loose reflections and actually burrowing under them, as suggested by Mayo, after the manner of a descending hernia.

The principle of mechanical irritation, the result of visceral gravitations, to which F. H. Martin ascribed a part in the production of membranous pericolitis, is, without much doubt, an important associated factor. Pilcher's view, which considers it the results of long-continued and oft-repeated mild infections of the peritoneal coverings of the cecum and appendix transmitted through the intestinal wall, and that of Gerster, which attributes the membrane to peritoneal reaction to infectious processes associated with chronic colitis, fit in well with the explanation of the embryonal origin of these membranes presented by Flint (*ibid.*), since their fusions, adhesions, and contractures are doubtless directly or indirectly due to inflammation. It is highly probable that mechanical irritations are causes of the clinical states embraced under the terms "membranous pericolitis" and "Lane's kink."

The final solution of the problem of the origin of Lane's kink and Jackson's membrane will, in our judg-

ment, be represented by the sum of the views of Flint, C. H. Mayo, Lane, Jackson, Connell, Coffey, Pilcher, Binnie, and Martin. In the meantime, however, much information of value can be gained by a study of the fetal peritoneal folds of Jonnesco, Treves, and Reid.

FOR DISCUSSION SEE PAGE 71

THE CO-RELATION OF APPENDICITIS, MUCOUS COLITIS, AND MEMBRANOUS PERICOLITIS

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It is only within the last few years that the medical profession has begun to grasp the real significance of colitis and to appreciate its effect upon the whole system and its relation to other local diseases.

The rôle played by the colon bacillus and other infectious micro-organisms within the bowel in the production of diseases of other organs, by the continuity of structure or through the blood- or lymph-channels, is now considered most important. The hitherto unexplainable headaches, neuralgic pains, malaise, and symptoms of general toxemia are being cleared up, and the mystery surrounding them dispelled. The relationship of a mucous colitis and a toxemia as cause and effect are found not inconsistent in many cases.

The frequency with which a membranous pericolitis is associated with a colitis or an appendicitis, as found at operation, has led to a more thorough study of these conditions. The symptoms of mucous colitis may be materially changed when complicated by appendicitis or *vice versa;* also, a membranous pericolitis may still further modify such symptoms. When all these conditions are present the symptoms may be attributed by the clinician to any one of the three, often through error, by not suspecting the second or third complication. In

certain cases we find that membranous pericolitis is an outgrowth of a colitis and an appendicitis occasionally resulting from one or both of these conditions. In others there may be a congenital form of membraneous colitis, but possibly less extensive.

I am not unmindful of the fact that the symptoms of any one of the above three conditions may be mistaken for gall-stone symptoms or a number of other conditions, and must therefore be carefully and systematically differentiated from them.

With these conclusions arrived at, and from not an inconsiderable experience, I am encouraged to present the views as expressed in this paper.

Mucous Colitis.—I am aware that the term *mucous colitis* is not an entirely satisfactory one under which to describe the phenomena associated with a disease of the mucous membrane of the colon characterized by an inflammation or catarrh with an excess of mucus passed; nevertheless, I have chosen this term as best fitting the object of the paper and under this head to recognize other forms, except special ones, as the different stages in the development of the disease.

The chronic variety is the one which concerns us most and will be the one usually referred to throughout this paper.

Cause of Mucous Colitis.—Mucous colitis may arise from repeated acute irritation of the colon, improper diet, poor or unclean food that may be more or less contaminated, constipation, congestion, or infection due to retention of feces. I believe also a great many cases are the outgrowth of repeated attacks of acute indigestion, diarrhea, enteritis, and colitis of childhood.

Goldthwait, Franklin H. Martin, and others have

placed particular stress on congenital body-formation as causes for visceroptosis, and therefore a potent factor in colitis. No doubt, the lack of proper tone of the abdominal muscles and incorrect body-formation, the lack of sufficient space in the upper abdomen, and the acquired increased amount in the lower favor visceroptosis with resulting distention, loss of muscle-tone, and stasis. With disturbances of the stomach and intestine there is a loss of flesh and absorption of retroperitoneal fat, which necessarily results in a further dropping of the viscera. Metschnikoff attributes much of the trouble of modern life to the fact that our concentrated diet has removed the need of so much large intestine.

Cause of Inflammation in the Colon.—Normally, the large bowel is less active than the small. The cecum is the weakest and widest portion of the large intestine. It is the seat of bacterial proliferation and gaseous production, making habitual distention and atony quite common. It is, as Arbuthnot Lane describes it, "the natural cesspool of the body." The antiperistaltic waves of Cannon, the great mucous surface, and the up-hill course of the fecal current are all elements which retard the progress of the contents of the colon. The development of a chronic inflammation of the mucous membrane in this portion of the colon necessarily follows. A rudimentary or excessive movable large gut may be a congenital element and favor stasis. It is more likely to occur here when visceroptosis is associated and angles are produced in the bowel, or constricting bands interfere with normal fecal circulation.

Symptoms of Mucous Colitis.—The passing of mucus in the stools is a very common symptom, but often not recognized by the patient until his attention is called to it. The mucus is seen sometimes in flakes, viscid and gelatinous masses, or ropy

strings. It is frequently accompanied by abdominal pain of paroxysmal type and alternate chronic diarrhea and constipation, the latter, however, is the usual condition. There are sometimes blood and pus in the stools.

There may be gastric distention, eructations, regurgitation, coated tongue, bad taste in the mouth, or erythema, vertigo, fatigue, insomnia, mental depression, anxiety, neuralgic pains, nausea, and occasionally vomiting. The nervous and circulatory symptoms are secondary, due to the absorption of toxins. The toxins seem to have a predilection for the vasomotor centers. There is a loss of balance between the splanchnic and the systemic peripheral circulation with too much blood diverted to the splanchnic area, producing congestion of the mucous membrane of the bowel with the result of too little blood in the systemic periphery, accounting for the cold extremities so commonly observed in these cases. The local irritation in a hypersensitive organism produces exaggerated reflex and nervous disturbances.

Savill reports 40 per cent of 103 cases of neurasthenia due to an incipient form of colitis.

Diagnosis.—The symptoms may resemble an attack of gall-stones, appendicitis, partial bowel obstruction, volvulus, acute dilatation of the stomach, and, occasionally, pyloric obstruction, nephritic calculi, hydronephrosis, and right-sided pelvic disease in the female. Macroscopic and microscopic examination of the stools makes the diagnosis conclusive by the presence of mucus, blood, or pus. It must be remembered that a prolapsed kidney is frequently present in a prolapse of the right colon, therefore, kidney and bladder distress are often present.

In bad cases the use of the sigmoidoscope should be universal to determine the presence of local in-

flammation or associated growths in the sigmoid. The Roentgen ray is also a very important adjunct for the purpose of diagnosis.

The rise of temperature and increased leucocyte count in appendicitis, as well as local tenderness and rigidity of the muscles of the abdomen, will characterize an appendicitis.

The soft doughy mass, gurgling in the right fossa, and the dull character of the pain, with little acceleration of the pulse and slight or no rise of temperature, are symptoms more prominent in a typhlitis or colitis involving the cecum especially. The pain may be general when the ascending and transverse colon are affected.

Complications.—De Langenhagen, Goldthwait, and many others have demonstrated that arthritis, gout, or rheumatism may have its origin in a mucous colitis. Neuralgia, nephritis, neurasthenia, and toxemia have been also demonstrated to have an origin in gastro-intestinal disease.

Colitis and Appendicitis.—In a series of 625 cases Geo. D. Kahlo reported 78 cases of appendicitis. Alfred Mantle of London says that intestinal surgery has shown that the mucous membrane of the colon and the appendix may be simultaneously affected, and he also states that, after a thorough investigation of many cases of colitis, mucous colitis is not infrequently an antecedent of appendicitis. Treves, in a report of a series of 45 cases operated upon for appendicitis in which the symptoms were unrelieved, declared that "one-fifth were suffering from persistent or relapsing colitis." He further states that "the association of colitis and appendicitis, is common, and that by extension of the disease, the appendix is involved." Lockwood says: "I do not know whether appendicitis can

cause mucomembranous colitis, but, on the other hand, I do know that mucomembranous colitis can cause appendicitis." Poynton cites three cases in children where this happened.

Caley has pointed out that the formation of concretions in the appendix is largely responsible for the graver forms of appendicitis and may be connected with disordered secretion of the mucous membrane of the cecum and appendix. Owen Williams says, "these concretions consist of calcium salts which are excreted by the intestinal mucous membrane and which enter into combination with the (saturated) fatty acids of the food to form insoluble compounds." "If formed in the appendix, they may set up local inflammation; or in the colon, catarrh with colicky pains."

Raymond Russ collected reports from Jordan, Kukula, Lanz, Sich, Polya, Feltz, Reisinger, Thomas, Henmeter, McWilliams, and Bitdorf; and from these reports he concludes that, as "primary typhlitis does occur and as the mucous membrane of the appendix is continuous with that of the cecum, secondary inflammation of the appendix will be frequent." A number of cases observed at operation have an inflammation of the cecum and ascending colon with involvement of the appendix; and in these cases mucous discharges from the bowel are frequent. The natural conclusion is, that the inflammation in cecum and colon has extended to the appendix. The extension of disease of the mucous membrane of the colon to the appendix may be aided by the lymph-channels as well.

Nearly every surgeon has seen attacks of acute appendicitis follow or accompany an acute diarrhea or colitis.

X-ray photographs have demonstrated that bismuth will enter the appendix from the cecum, and therefore we must conclude that the bowel contents will enter

an appendix from the cecum, and inflammation result or appendicitis supervene.

Membranous Pericolitis.—It is not at all unlikely that we shall soon revise our opinions and ideas as regards acute and chronic inflammatory diseases of the right pelvis. We may reaffirm many of the ideas of older writers and recognize typhlitis and perityphlitis as having a clinical and pathologic entity.

The ante-mortem treatment of today in contrast to the older "post-mortem" attention given cases of "peritonitis," "inflammation of the bowels," and "obstruction," formerly considered fatal maladies, has demonstrated that, in most instances, the appendix is the offending organ.

The removal of the appendix has become quite universal and "so easy" in simple cases that the incompetent general practitioner tries his hand at it occasionally. This has led to unnecessary operations and the unwarranted removal of the appendix. It was considered a mark of great surgical dexterity and skill to take out the appendix through an incision two fingers' width in length and in two minutes. This favored rapid operating and greatly facilitated the surgeon in increasing the number of operative cases per day and dollars per month.

From this method the public has suffered and the profession has been criticised. Many patients operated upon were not relieved of their symptoms. They returned with their old complaints, as soon as the psychological effect of the operation had passed. These failures have told upon the reputation of the surgeon and have discredited surgical work in general, therefore reasons other than "neurasthenia" had to be found for the continuation of the patient's suffering.

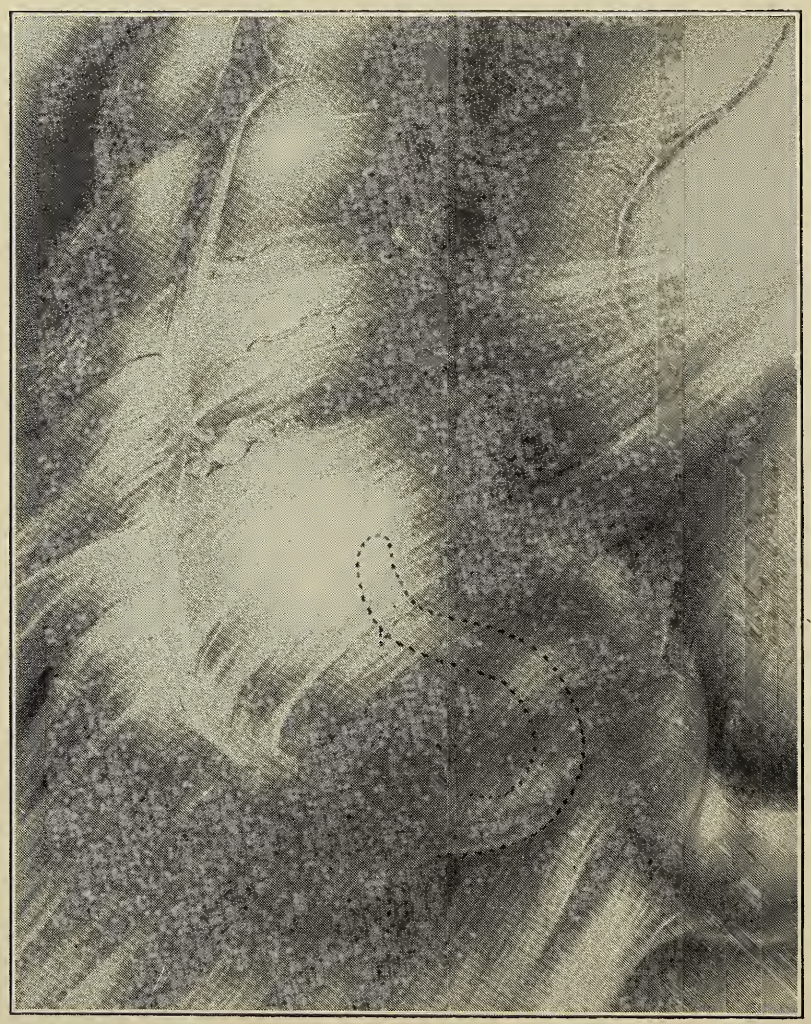
Larger incision are now made—often median openings—and a general investigation of the abdominal and pelvic organs is carried out.

The frequency with which the cecum, appendix, and colon were found, at operation, to be enveloped in a second layer of tissue, caused careful and observing surgeons to ascertain the true significance of this membrane. Many operators noting, in the course of abdominal operations, the crippling effect of this membrane upon the bowel, proceeded to separate some of *these fibers, releasing the bowel from the constricting bonds.* English surgeons years ago wrote more or less upon what they termed *typhlitis* and *perityphlitis,* and had they not been thrown off the track by the "appendicitis craze" and had they pursued their studies along this line, the profession would have been enlightened earlier.

Virchow, Binnee, Willms, and others have described various irregularities and abnormal conditions surrounding the colon with a movable cecum. Dr. Jabez B. Jackson, in his excellent paper read before this Association in December, 1908, first described the condition as a clinical entity. The pathological, as well as the surgical, consideration of the disease awakened much interest, and more careful investigation has been going on since that time, until now we have arrived at some definite conclusion in regard to the disease.

Membranous Pericolitis, the Result of Colitis.—I need not enter, in this paper, into a description of the membrane, as Gerster, Jackson, Pilcher, Harris, and others have given us such a clear understanding of it that nearly every surgeon is now familiar with the appearance in the various forms in which it is found. I shall mention, however, a few points of special interest and give some personal observations.

Case 15. Appendix kinked by membranous pericolitis.



Case 29. Membranous pericolitis with Lane kink.

I have found in most cases that the membrane and fibers with the blood-vessels seem to radiate from the parietal peritoneum onto the colon fan-shaped, or to run at right angles to the gut and often constricting and greatly narrowing the lumen of the colon in such a manner that hernia-like protrusions are observed between these constricting transverse fibers, producing an unevenness greatly accentuated in places, the walls being very thin and even transparent at times. Upon the liberation of these transverse fibers, the cecum or colon unfolded and presented more of a uniform dilatation. I have observed this membrane completely encircling the cecum or ascending colon, involving the hepatic and splenic flexures and often the transverse colon and sigmoid.

The ascending colon is often rotated outward to an angle of 45° or more by the contracting and constricting fibers. The membrane covering the sigmoid is less vascular and has been found covering this portion of the colon, constricting it or binding it down in an abnormal position. My attention was first called to the sigmoid involvement about eight years ago, when I began separating the adhesions here allowing the bowel to assume a normal shape and position. It was observed in women, particularly, that the fibers seemed to extend over and in front of the broad ligaments, pulling the sigmoid over at this point.

My observations were later directed to the hepatic flexure, as well as to the cecum and transverse colon, and the operations were each time more and more carefully and extensively performed with surprisingly good results.

It was Dr. Jackson's paper, however, which gave me inspiration and caused me to fix upon the condition a special significance and to do a more complete and careful and systematic separation of these fibers

when called for. I have tried to recognize the importance of after-treatment to clear up what I thought was often the causative factor, namely, the colitis.

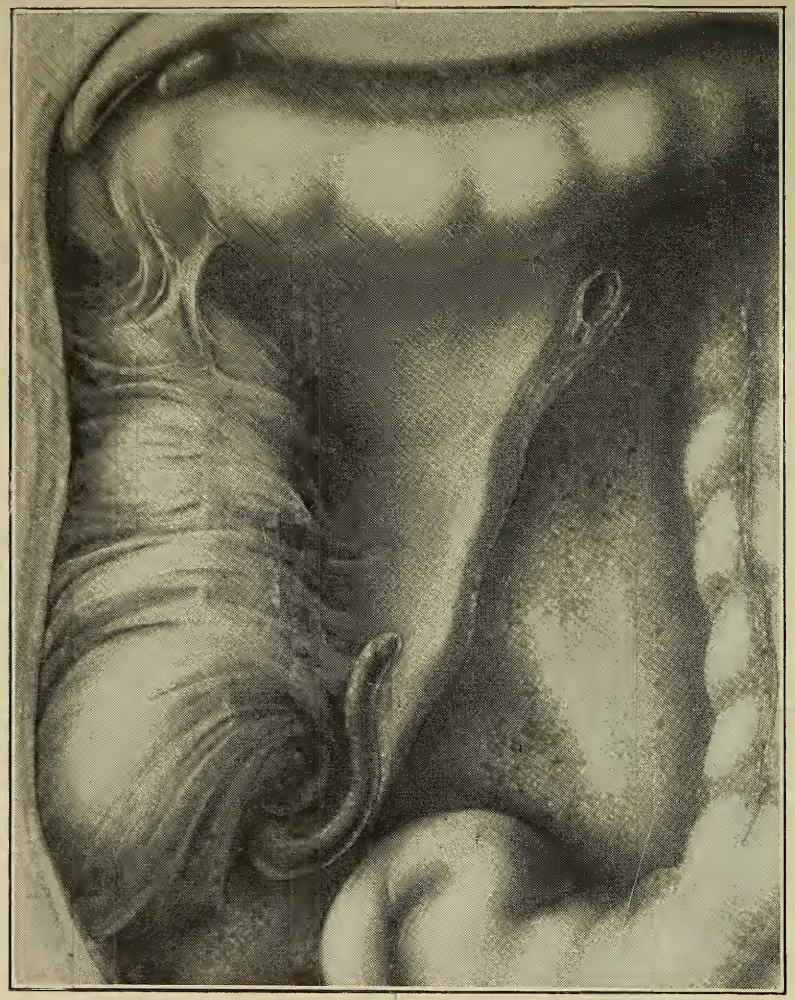
Etiology.—The etiology of this membrane is in an unsettled state. Authors differ materially in their opinions. Chas. H. Mayo suggests that these bands and films may be due in some cases to the late rotation of the bowel and descent of the cecum. Willms has suggested that an unduly mobile cecum with ptosis may lead to distention, atony, and inflammation of its walls, conducive to fecal stasis. Lane believes the membrane is the result of the tendency of the cecum and ascending colon to fall, due to the upright position, and that these lines of strain are crystallized into adhesions, forming bands which tend to fix the structures.

Alfred Mantle says, in his article entitled "Mucous Colitis and Its Relation to Appendicitis and Pericolitis:" "While we have for years recognized the condition of perityphlitis, we have not so readily accepted the fact that any part of the colon may have an inflammatory area starting in the mucous membrane and involving the peritoneum, as in perityphlitis." Rolliston, Treves, Pilcher, Jackson, Gerster, and Frazier have expressed the belief that the primary cause of membranous pericolitis is the chronic colitis.

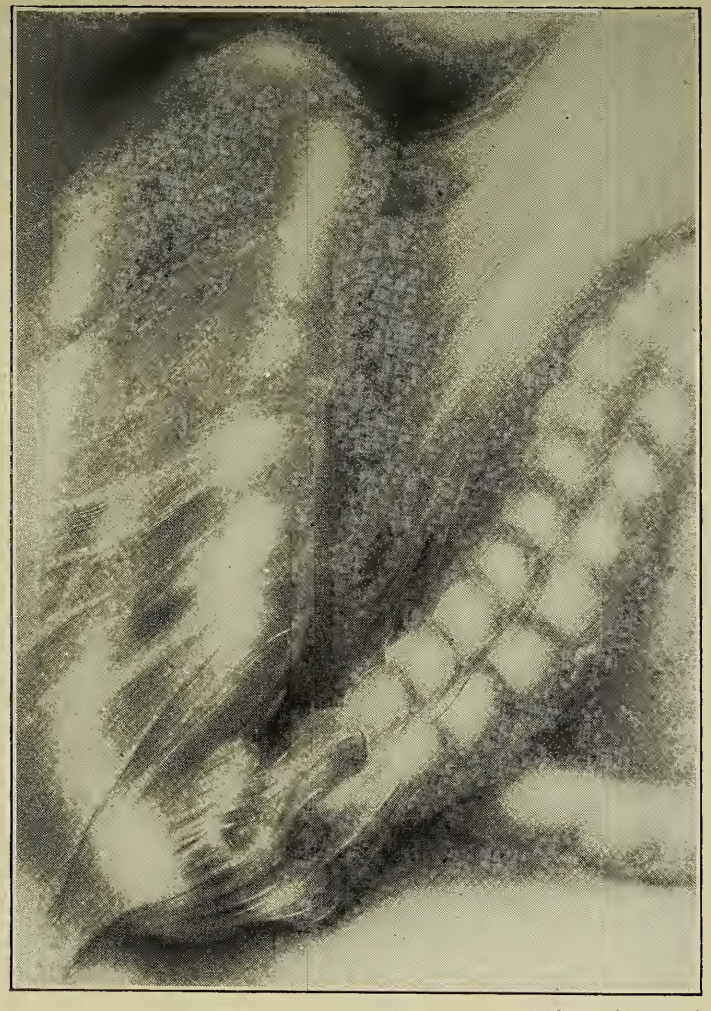
I am convinced that there is more than one etiological factor concerned in the production of this membrane, as found in the various grades of inflammation and forms of this trouble. Cecum mobile or improper rotation of the gut, lax belly-walls with general visceroptosis, Lane's kink; disease of the gall-bladder, pelvic inflammation in the female, prolapsed cecum, chronic constipation, chronic dilatation and atony of cecum, bands and constrictions and kinks of the colon with stasis—all have a bearing upon the etiology of

Case 41. An occasional type of the crippling effect of the membrane.

Case 55A. Long appendix extending to the gall-bladder, covered by membrane; a beginning kink at the hepatic flexure.



Case 57. Not an unusual type of membranous pericolitis.



Case 62. Double-barrel shot-gun deformity of colon. Appendix included in membrane.

this membrane. A thinned-out bowel-wall and the migration of the infectious micro-organisms through the wall of the gut seem a reasonable explanation for the reaction of nature to irritative influences by the calling forth of the opposing forces to prevent further dilatation and infection of the surrounding tissue by the production of this adventitious membrane.

It is impossible to say what is the normal range of motion of the cecum in the healthy individual. Properly speaking, there is no such thing as a mesocecum, so that a cecum of normal size floats freely and enjoys a considerable amount of movement.

Dr. Dreyer has reported 67 per cent movable cecums at autopsy, and only 11 per cent of women are free of the condition, which leads us to say that, if only 11 per cent of women have a fixed cecum, that which has been considered the normal state may be the abnormal, and that a movable cecum is not by any means inconsistent with good health.

A loaded cecum no doubt will cause a prolapse and stretching of its supports and resembles a mobile cecum. We may find the cecum firmly fixed in the pelvis and a dense membrane surrounding the cecum and colon. We proceed to liberate this cecum in our treatment of the disease, and find the colon then to present more of an even and normal surface. Some have considered cecum mobile as the first cause and perhaps the only one. If such is the case why should not a similar membrane occur on a movable loop of the small intestines especially where there is some stasis. Furthermore, we observe that this membrane is present in other portions of the alimentary canal where motion is of a limited extent, such as the sigmoid, and hepatic and splenic flexures. Also we may occasionally find a membrane present similar in appearance but thinner and more delicate upon the small

bowel where an enteritis antedated the operation or was present at the time of the operation or when there has been stasis due to distal adhesions, as Lane's kink, volvulus, or partial obstruction.

Intracolonic tension, produced by distal kinks and bands causing partial obstruction with stasis, fecal, and gaseous distention, results in improper drainage; infection follows from retained mucus and feces. The infectious micro-organisms are forced through the thinned-out cecal or colonic wall and invade the contiguous peritoneal membrane. Nature rebels against the insult and immediately throws out a protecting wall or membrane, which varies in density and extent in direct ratio to the virulence of the infection and the duration of the disease.

We see a similar membrane present at the pyloric end of the stomach, which is probably produced by some near-by infection, as a diseased gall-bladder, or from an old ulcer with some constriction present which has caused gastric stasis, and often little or no demonstrable lesion can be found outside of the membrane.

It has been demonstrated by Franke that the cecum and ascending colon are connected by a chain of lymphatics with the right kidney; and he states that bacteria are able to pass indirectly from the intestine into the lymph-vessels if there is even a slight lesion of the intestinal wall. In the production of membranous pericolitis, there is evidence that such a transmigration of the bacillus through the walls and into the lymph-spaces does take place frequently. Pilcher reports several cases to substantiate his belief that pathological conditions of the kidney are directly consequent to preceding pathological changes in the cecum and ascending colon.

It is also possible for the gall-bladder to become in-

fected through the blood and lymphatic circulation with the production of gall-stones. Not infrequently do we find adhesions around the gall-bladder, probably due to infection of that organ through the blood and the transmigration of the infectious micro-organisms through the wall of the gall-bladder to the surrounding structure, producing peritoneal reactions with adhesions.

If a kidney may become involved on account of a colitis and a gall-bladder becomes infected and covered with adhesions around its fundus, why is it not possible for this membrane to form on account of the transmigration of infectious micro-organisms through its wall?

Symptoms of Membranous Pericolitis.—The symptoms of this condition vary according to the location of the membrane, the amount of constriction and stasis, and the consequent toxemia. A modification of symptoms is often dependent upon diet or exercise of the individual. Muscular spasm and enterospasm are noticeable in cases where there is an interference of the bowel function and limitation of its caliber. All the symptoms dependent upon an associated colitis, stasis, and auto-intoxication, such as malaise, periodical headache, backache, nausea, vomiting, and neurasthenia, may be present, and when there is an exfoliation of the mucous membrane and ulcerated condition of the colon there may be a leucocytosis with fever and chills. There is a loss of weight and strength of the general muscular system. The color of the individual is usually sallow and presents a picture of a person suffering with the so-called bilious attack.

In nearly every case there is more or less pain in the abdomen. This is especially noticeable in the region of the cecum and very often near the gall-bladder

simulating gall-stones. In bad cases, especially where there is a kinked condition at the hepatic and splenic flexures or displacement of sigmoid due to bands pulling the bowel out of place, the pain may be general. The soreness along the colon may be constant, but there are acute exacerbations that are very severe at times, the pain being located so often near the appendiceal region during the acute attack that many cases are referred to the surgeon for operation for the removal of the appendix.

Constipation is the rule, although there may be an irregularity of the action of the bowel, alternating between diarrhea and constipation. Nausea and vomiting are not infrequent. Dyspepsia is the most common complaint, there being hypochlorhydria, gaseous distention and eructations, and distress following meals. The cecum may be greatly distended with gas at times, and may give a tympanitic note upon percussion. Evacuation of the bowel and the expelling of gas usually give relief. Mucus usually accumulates in the colon, especially in the region of the cecum and possibly the transverse colon and sigmoid. Mucus may be expelled in large quantities or come away at the usual evacuation of the bowel. This mucus may be ropy or stringy or in flakes or gelatinous masses. It occasionally contains blood, pus, and, in a severe type of the disease, membranous exfoliation of portions of the colon. Pilcher describes the pathological complex as a defect of peristalsis, the mechanical action of the bowel being rather obstructive from bands, enterospasm from persistent irritation, and auto-intoxication.

Membranous Pericolitis and Appendicitis.—Innumerable patients have been operated upon in the past for appendicitis and are being operated upon today with little or no benefit. The microscope is revealing

our mistakes and the futility of such surgical work where we have not gotten at the true causes of the symptom-complex. The button-hole incision for the removal of the appendix is an operation of the past. Very few appendices should be removed now through a small incision unless we are certain that the disease is confined to this organ alone, or that it is of such an acute type that the appendix should be alone attended to. Therefore, the indiscriminate removal of the appendix without a complete diagnosis and thorough investigation will seldom be seen in a few years. Very few statistics have been published of the end-results of chronic appendicitis. Graham and Guthrie (*Jour. of the A. M. A.,* vol. 54, p. 960) reported a series of chronic appendicitis of the dyspeptic type, where approximately five per cent were not cured by operation and ten per cent had occasional return of symptoms. Stanton, in 1911, reported, in a series of 100 cases, thirty-six where the end-results were not satisfactory or other lesions were found as the cause of the symptoms. Willms, before the 40th Congress of German Surgeons, expressed the opinion that the diagnosis of appendicitis was often made too lightly. He estimated that at least 30 per cent of so-called cases of chronic appendicitis are cases of chronic constipation with a movable, dilated, and prolapsed cecum. Klemm, Hausman, Stierling, and others have made similar statements.

A membrane forming around the caput ceci may bind the appendix down, producing angulation and degenerative changes in its walls. When this membrane involves the terminal portion of the ileum it may prevent the normal delivery of the contents of the ileum into the cecum, producing what is ordinarily termed Lane's kink. Chas. H. Frazier, in his recent article published in the *Annals of Surgery,* emphasizes the necessity of studying each individual case so as

to avoid the removal of the normal appendix. We might add here that it is not the removal of the normal appendix that is to be regretted so much as the fact that the real cause of the symptoms, namely, membranous pericolitis and an underlying colitis, was not benefited by the removal of the appendix, and the removal of the appendix cannot free the patient from a long-continued colonic inflammation and suffering.

No one who has operated many times for the removal of the appendix can have failed to note the frequent coexistence of a more or less extensive congestion of the cecal peritoneum, a true typhlitis.

I have for some time appreciated that in occasional instances this co-existing perityphlitis and pericolitis was of more importance than the appendicitis itself.

Treatment of Mucous Colitis, Membranous Pericolitis, and Appendicitis.—In a case with symptoms, e. g., passing of mucus, colicky pain, and no fever, it has been my habit to clear up the diagnosis by the various methods now at our command, and to determine, if possible, whether the case is one for medical or surgical treatment. In cases with mild forms of the trouble with evidences of colitis, colicky pain, acute exacerbations of chronic inflammation with more or less stasis, colon-distention, and auto-intoxication, and where visceroptosis is present, the colon lavage and olive oil enemas given in the knee-chest position and right recumbent position have been employed quite constantly with altered diet of such a character that it produces little or no gaseous or intestinal distention and discomfort. The administration of intestinal antiseptics, such as beta-naphtol, salol, boric acid, sodium salicylate, belladonna, bismuth, and charcoal, and systematic exercises and massage to develop a more capacious upper abdomen and a reduced lower abdomen by the proper tone of the abdominal muscles, give good results.

Franklin H. Martin explains, in a recent article, a very elaborate system of gymnastics that I had been advocating in a modified degree for several years. An increase in the cavity of the upper abdomen is accomplished by elevating the foot of the bed whenever the patient lies down and often during the whole night, keeping the colon fairly empty by daily flushing and the wearing of a lower abdominal support or a properly fitted corset in females while the patient is erect, this being placed before rising. Attention on the patient's part to moderate exercise, warm clothing, and avoidance of exposure to undue cold will give the greatest comfort, and by attracting blood from the splanchnic area to the periphery will relieve the affected bowel. Care of diet, by avoiding all articles of food the residue of which may irritate the mucous membrane, must be strictly observed.

It is only left to say that the treatment by this method of chronic constipation and mucous colitis, associated or not with appendicitis and pericolitis, has been quite successful, as well as educational to the patient.

I am confident that the unnecessary operations of this kind would occur much less frequently if every colitis patient were properly treated from the incipiency of the case, and that the results of these operations, when indicated, would be far better if more attention were given to the after-treatment of the colon.

In a certain percentage of cases, however, in which the symptoms are so palpably due to kinks, flexures, and adhesions owing to a membranous pericolitis, surgical intervention is called for; and in such cases we are often surprised to find under what difficulties the intestines had been functionating, yet doing comparatively satisfactory work, but after their release we find that they soon regain their tone and muscle-activity

to such an extent that the patient is relieved of the stasis, constant infection, malaise, headache, and general debility. In dealing with the fibrous bands, *per se,* it becomes a matter of experience and judgment as regards the handling of them. It has been my habit to liberate the colon, including the sigmoid hepatic and splenic flexures where they are involved, and to dissect off this membrane by blunt dissectors, gauze, and scissors, and to leave the underlying tissue in as normal a state as possible, free from bleeding surfaces and raw areas. When the membrane is closely associated or connected with the bowel, as in a congenital form, it is not removed excepting the constricting bands.

In order to get at the part of the bowel that is particularly affected, our incisions must be made in such a manner as to accomplish this.

In case of general pain our incision must necessarily be a median one; but where the symptoms point quite positively to a right-sided involvement (the usual case), the right-rectus incision or one along the linea alba, with an oblique incision inward at the upper portion of the abdomen, and in some instances the Bevan incision will reveal all of the tissue affected.

The stomach may be pulled out of place, resulting in a kink at the pylorus or a portion of the duodenum because of adhesions of the transverse and ascending colon when there is dilatation and interference with the motility of the stomach. Upon the liberation and straightening out of the colon, the stomach, if pulled out of place, regains the normal position and function in most instances. The appendix is frequently found held fast by bands and often distended and twisted so that its drainage is imperfect. Portions of the ascending colon that are held and constricted or twisted by bands should be liberated, and all bleeding points

ligated and raw surfaces protected as much as possible. Longitudinal plication operations of the cecum, when greatly dilated, should be performed. This lessens the size of the bacteria-holding sac and at the same time strengthens the walls of the intestine and makes stagnation and absorption less possible.

One thing is certain, namely, that cases requiring surgical intervention must be followed up after the operation in a rational and intelligent manner. Simply removing the membrane and not directing our attention to the cause, is futile and only invites a return of the trouble. Attention should be given the diet and proper exercise to tone up the lower abdominal muscles, and the same treatment should be followed out after the operation that is inaugurated in the non-operative type or stage of the disease. The accompanying colitis must be cured, and by this means the cause of the pericolitis relieved and future bands prevented.

When a cecum mobile is encountered and not already fixed posteriorly, I believe it advisable to fix it so as not to allow the possibility of the too close approximation of the cecum and the hepatic flexure of the colon.

Where nature has in a measure by the formation of bands fixed the cecum, I am in the habit of confining my attention to the fibers constricting the colon or cecum in any way, for I believe the others will disappear under proper treatment of the colitis. Where the stomach and transverse colon are especially prolapsed I have in some instances shortened the gastrohepatic ligament and occasionally fixed the omentum to the abdominal wall, similar to Coffee's method, with very satisfactory results. This method lessens the possibility of a refixation of the omentum and transverse colon to the ascending colon. Patients are us-

ually put on the left side with the foot of the bed elevated when the cecum and colon have been liberated to prevent a close association of the diseased walls.

CONCLUSION.

1. The colon bacillus is a most deadly enemy of the human race when it escapes from its normal habitat into the peritoneal cavity.

2. Owing to the dietary habits and the upright posture assumed by man the propagation of the bacillus is favored.

3. The congenital body-form and the imperfect development of the large intestine, as well as insufficient support and pressure of clothing, favor visceroptosis, and visceroptosis favors stasis.

4. Repeated attacks of colitis in children may terminate in a chronic form of disease to be manifested in later life.

5. Intestinal stasis causes a multiplication of the colon bacillus within the colon.

6. The weakest portion of the colon, namely, the cecum, owing to its anatomical location, is the natural receptacle for retained feces, mucus, bacilli, and other contents of the bowel.

7. The two fixed points, namely, hepatic and splenic flexures, inhibit fecal circulation.

8. The reverse peristaltic waves and the "uphill" course of the first part of the colon are prime factors in the production of stasis in the cecum.

9. When stasis or intracolonic tension exists in the presence of retained infectious micro-organisms, the law of osmosis is put in operation and a transmigration of these organisms through the walls of a thinned-out colon occurs.

10. There is always a reaction on the part of tissue thus invaded, and barriers are offered to protect surrounding structures.

11. Without the colon these barriers are manifest in the form of a veil-like membrane radiating from the parietal peritoneum to and over the area of the colon most affected.

12. This membrane may not be placed uniformly over the colon or cecum, but often in an imperfect and crippling manner.

13. The pressure of these bands may cause degeneration and scarring of the intestinal walls with contraction, resulting in thick bands or dense adhesions which produce obstruction and secondary stasis.

14. When these fibers hamper the action of the large gut they are a menace to the health of the individual and must be removed.

15. The appendix is often caught in this veil-like membrane or dense bands with resulting constriction and degeneration of its walls.

16. When the freedom of action of the appendix is thus interfered with, bacteria are retained and further degeneration and infection result. Acute attacks of appendicitis may supervene following chronic symptoms of the disease.

17. A chronic colitis may result, sooner or later, in a membranous pericolitis and demand surgical intervention to release an imprisoned or restrained portion of the colon. The results of such an operation, however, depend, to a great extent, upon consistent dietary and general hygienic measures following.

18. Button-hole incisions for appendicitis should seldom, if ever, be employed.

19. Surgeons must take a more comprehensive view of diseases of the abdomen and especially of the

right side, and cease to focus their attention on the appendix alone.

20. A careful observation must be made of all the abdominal organs at the time of the operation and a general view taken; and with a clear understanding of the clinical symptoms and a careful investigation of the pathological conditions requiring attention, and with an intelligent interpretation of these, our results will be quite satisfactory.

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DISCUSSION OF THE THREE PRECEDING PAPERS

Dr. Van Buren Knott (Sioux City): Much has been said and written regarding Jackson's membrane and other anomalies occurring in the neighborhood of the cecum. I was much interested in Dr. Eastman's paper because it conveyed to me much new information.

The papers of both Dr. Connell and Dr. Eastman emphasized the point that there is much difference of opinion in the profession concerning the so-called Jackson's membrane. I have long thought that the membrane was more a congenital anomaly than a clinical entity, having noticed it many times when the abdomen has been opened for other conditions when no symptoms existed which could in any way be attributed to the presence of the membrane.

Soon after the subject was first discussed, I made it a point, in all abdominal operations which would permit, to look for this membrane, and I found it with great frequency, attended by total absence of symptoms. In many instances the adhesions do not limit cecal motility, and there is no reason to suppose that this fold, present so frequently, as is beautifully shown in the fetuses exhibited by Dr. Eastman, is of any other than congenital origin. Dr. Eastman tells me that he has found it in a very large percentage of fetuses examined, and I am sure that it exists on through life and may be demonstrated in a much larger percentage of cases than one would believe unless convinced by examining the cecum whenever feasible.

The bloodless fold of Treves and the genitomesenteric fold, also congenital, are frequently responsible for more or less kinking of the ileum or the cecal margin and are not of inflammatory origin. Many times have I shelled out the caput coli, as described by Dr. Eastman, from the pocket formed by the fold of Treves, and it came out freely, not being tied down by adhesion.

I believe the literature pertaining to this subject has been enriched by the papers of Dr. Eastman, and I am very glad to have had the privilege of hearing him.

Dr. W. D. Haines (Cincinnati, Ohio): What little I have to say will pertain to the paper of Dr. Benjamin on the correlation of appendicitis and other pathologic lesions in the abdomen, such as mucous colitis, pericolitis, etc. Mucous colitis is a pathologic junk-heap into which we formerly dumped all that certain parcel or lot whose etiology was placed under the general caption "idiopathic." We now know there is a definite cause for

colitis. It is an infection of the intestinal wall, and it behaves very much as other mucous-lined surfaces do when they become infected, the difference being principally in the mechanics of the part, whether the infection be in the gall-bladder, the appendix, or the gut itself. I certainly agree with the doctor, that unless you can find the initial pathological condition and remove it in dealing with membranous colitis, you are going to fail in your attempt at cure. If you are operating on a case of appendicitis, and the case proves to be one of intestinal infection, you are going to fail, in all probability, although great benefit may be derived from the appendicectomy.

In the operative treatment of this type of case we insist upon one thing, and that is, making an incision sufficiently large to see what we are doing. If the incision is of ample length, it will permit us to see intelligently that which lies before us, and we can remove, to the best of our ability, the cause which is producing the symptoms of which the patient is complaining.

Dr. Jabez N. Jackson (Kansas City, Mo.): I think probably there are present a number of Fellows of this Association, who will remember my original presentation of this subject before a meeting of the Association in 1908 at Minneapolis, and they will remember one thing I emphasized at that time, namely, the term "membranous pericolitis" was descriptive without any acceptance on the part of the essayist of the significance of an inflammatory condition as a necessary etiological factor in its production. It was described as a *pericolitis* because it was about the colon, and I prefer that term, and simply an inflammatory idea was suggested on account of a large number of intestinal groups of blood-vessels with vascularity similar to those found in inflammatory conditions.

The main thing I have endeavored to force upon the profession is the recognition of a pathological condition, and I say *pathologic* after using the term "abnormal condition," whether it be congenital, inflammatory, or what not in its origin,—a condition which is found about the right side of the colon in a very definite percentage of cases; a condition which is clearly definable and describable; a clinical entity which presents an absolutely characteristic train of symptoms.

Dr. Knott, in his discussion, referred to the fact that he had operated upon cases in which the pericolic membrane was found without any symptoms whatsoever indicative of its presence, and that is undoubtedly true. That has been the observation of probably all of us. The existence of this fold or membrane may, or may not, be sufficient to cause symptoms;

but when the membrane has developed to the stage or condition in which we have fixation and angulation of the colon, which is so generally characteristic, we then get a group of symptoms which make the condition an absolute clinical entity.

Since the presentation of our original mooted paper, a considerable amount of discussion has taken place in an effort to arrive at a correct interpretation of the etiology of this condition, and, I am frank to say, I consider that the most important subject the profession has before it today, because upon the solution of the question of the etiology must ultimately rest the direction of our therapeutic efforts. Now, in a general sense, the theories which have been brought forward in trying to explain the condition of this thing, have grouped themselves, first, under what may be known as a congenital theory. Probably the first man to direct our attention to any congenital idea was Dr. Mayo, who assumed that with the colon in its descent the peritoneum failed to force itself down to the viscus, and remaining loose it received a peculiar vascularity from the parietal vessels and presented the characteristic picture we have found.

Another theory of congenital origin that has attracted some attention, is that which was presented to me by Keiller, an anatomist in the University of Texas, which was supported by a Frenchman from Lyons, France, and has been supported recently by Flint, a professor in Yale, and that is this: In certain cases before the cecum has completed its rotation, where the omentum comes across and drops down, as it does, particularly from the transverse colon, we may have a portion of the omentum attached to the colon, and particularly above the cecum, and then as the cecum descends this membrane is dragged down over it and represents a portion of the original omentum. That idea is exceedingly interesting from the fact that we have found in all cases of true pericolic membrane a membrane that extended over the entire ascending colon up to and involving always the hepatic flexure and passing across onto the transverse colon to the right margin of the omentum, and in that way very largely continuous with the omentum. We have also seen cases, such as one of Dr. Eastman's pictures shows, where the omentum is attached clear down, fixing the ascending colon to the cecum. I have seen several times the attachment above here (indicating) and at the lower right-hand corner of the omentum attached to the gut.

Then we have to deal with another congenital theory, and that is the theory exploited by the Germans, if we can look upon

the cecum mobile as a general plan. As a matter of fact, it has been recognized that, in the vast majority of cases in which we have a mobile cecum producing symptoms, it is not primarily a mobile cecum with which we are dealing, but we have what is an attendant condition of the pericolic membrane, with an obstruction which we noted in our paper as a real cause down to the cecum, but rarely ever covering the cecum. When the obstruction is above the cecum we have dilatation of the cecum, which is called *atonic dilatation.* In the vast majority of cases these dilated ceca which give rise to symptoms are the ones which are a part of the general picture of membranous colitis.

We have the physiologic explanation of Dr. Lane, he believing that this is the way physiologic membranes form, with a tendency to abdominal displacement of the colon; in other words, that it is a factor in the production of ptosis. I am frank to say I have never paid much attention to the theory of Lane for several reasons. In the first place, because in these characteristic conditions there is never any ptosis of the colon. There is ptosis below the membrane with dilatation of the cecum. As a matter of fact we find the hepatic flexure unusually highly located, sometimes even with the membrane passing over to the gall-bladder and under the surface of the liver. We will say this: If this is an effort on the part of Nature to prevent ptosis, she has done a poor job, because a part of the colon, which is in relation to ptosis of the cecum, is not invested with membrane and not sustained by it.

We have likewise the inflammatory theory, which was originally accepted. This was that there were masses of adhesions from preëxisting bands or inflammatory troubles outside of the gut itself, but in the neighborhood. That theory has been generally exploited on account of the failures following efforts to relieve the condition.

Then comes the theory that has been presented by Gerster, it being an outward expression of an internal infection as colitis. At the present time I am in doubt in my mind as to what is the exact etiologic factor in the production, whether it may not be, as a matter of fact, a number of types of membrane, some congenital, some inflammatory. If I could accept one theory which would be satisfactory to me, I should prefer the congenital theory, because, if the congenital theory is correct, the surgical treatment would be simple. Mechanical treatment should be directed to the removal of these congenital anomalies, while, on the other hand, if it be inflammatory, from colitis or

anything inside the gut, if we think it able to produce destruction outside, we have something on the inside that must be further treated.

Dr. W. W. Grant (Denver, Colo.): We have beautifully illustrated here by Dr. Eastman the fetal membrane as it appears even in the adult in some of these cases, and yet it is the clinical feature of this question that concerns us most, and without which the investigations are purely academic. My own experience confirms what Dr. Jackson has stated in these cases, that there is generally no ptosis of the colon.

Regarding the Lane theory: It seems to me that Mr. Lane has put the cart before the horse, and I say this with some reluctance because of the very distinguished character of this gentleman and the place which he holds in the minds of American surgeons. I have found in some of these cases absolutely no local symptoms, so far as the appendix region, or the ascending colon, or the descending colon is concerned. I have found simply stomach-symptoms, and in these I have succeeded by curing the local conditions, which are found by operation, sometimes an exploratory operation. It seems to me, the view of Gerster that while it is not pathologic in the infant or fetus, yet it does become so in the adult, is correct, and it is the pathologic condition that interests us more than anything else. I would then especially call attention to the absence of local symptoms in many cases. I have found in a recent case, which I reported to the Clinical and Pathological Society in Denver, a most interesting local condition of the appendix, and in three years there were no local symptoms other than those referable to the stomach, i. e., hyperacidity and pain in the stomach. An exploratory operation showed absolutely no disease of the stomach or of the gall-bladder, but the appendix doubled on itself was covered by this membrane, which partly covered the cecum, looking exactly as it does in the fetus, except that there was a great deal of congestion of the blood-vessels covering the cecum. The removal of the appendix cured this patient in two months. He gained ten or fifteen pounds in weight. It shows that there was a pathologic condition in this case, and, it seems to me, the view of Gerster, of a slow infective process from the intestines, is sound and is the real explanation of the cases that come to the operating table.

Dr. Daniel N. Eisendrath (Chicago): I believe that Dr. Jackson is right and that Dr. Eastman is right, and I believe that discussion is gradually going to bring order out of chaos.

I want to make the last point of my discussion first. I would suggest that in our discussion of this subject we watch our cases for the next year or possibly two years, make notes of our observations at the operating-table, and follow the cases along to see how much benefit they have derived from the various methods of operation and how much necessity there is for any operation in a great many of these conditions.

I wish to take issue, in the first place, with one point Dr. Jackson made, although I did not know he was going to make it. Beginning last April, when I went on the surgical service at the Michael Reese Hospital, I made notes in a great many of the cases of so-called movable cecum and of ptosis of the ascending colon and transverse colon; and I have been following a number of these cases right along. In the first place, this chart which I show you is a contradiction of what Dr. Jackson has just stated, and that is, the Jackson membrane does not form in ptotic colons. Here is a picture (illustrating) that was made within fifteen minutes after the operation, but with no thought of any reference to Dr. Jackson's views now, and you will see that the cecum lies between the bladder and the right pelvic wall. As has been stated, the Jackson pericolic membrane covers the ascending colon down to the cecum. In this case there were absolutely no symptoms referable to this ptotic condition of the colon. The colon and the entire ascending colon and cecum were in the condition that has been described as cecum mobile, or could be called in these cases *complete ptosis of the cecum.*

I will pass these illustrations around that you may note the cases one after the other. You will notice the same condition without the Jackson membrane.

I would suggest, in order to get a little light on this subject that each one of us take a number of charts like this and have our students, or whoever notes the operations and makes reports of them, see how far the cecum can be moved, whether the Jackson membrane is present or not, and whether there is a prolapsed condition of the cecum. Such observations will throw a good deal of light on this subject in the future if they are carefully made.

Dr. Jackson is partly right, and Dr. Eastman is also. We have been mistaken about a great many of these membranes. Many of these cases which have been called Jackson's membranes are undoubtedly the fold or veil of Jonnesco and of Reid or the bloodless fold of Treves. We ought to get order out of this matter, and the way to do it is to learn that there

are certain membranes which are embryonic and do not play any pathogenic rôle, while others do so.

I do not believe there is any doubt in our minds today that in many of these operations there are certain cases in which the Jackson membrane may be called a double-barreled shotgun action, as described by Dr. Connell. To illustrate this I wish to recite a case on which I operated for a typical chronic appendicitis three years ago. The patient came to me with symptoms referable to the appendix. The patient had been operated on before. We noted in the original operation that she had a well-marked Jackson's membrane, but no kinks were present. Three years later, when I examined the case, we made a drawing at the time, and there was a typical double-barreled shotgun action, but evidently pathogenic, because this developed under our eyes. We examined the ascending colon three years before, and this was entirely a true condition caused by the Jackson membrane.

Dr. Jackson has referred to the rôle which the cecum mobile plays. I believe cecum mobile is in a great many cases one of the chief factors, but I cannot quite agree with him that in every case we need to have a fixed cecum. There are a great many cases where there is a movable cecum from its pendulum-like action. It becomes kinked close to the hepatic flexure, and in this way produces coprostasis.

I do not believe in a great many cases the suggestion which has been made of fixing the cecum or plicating it, is getting at the underlying trouble. In a great many cases where the colon is close down in the pelvis, nothing short of operative interference in the way of fixation will do any good.

Dr. Warren A. Dennis (St. Paul, Minn.): It occurs to me that this question may be definitely settled by the co-operative work of a considerable number of men. If we all keep definite records in all our operative cases of the findings in the region in question, and compare these findings with those present when for any reason any of these cases require a later laparotomy, the data will be at hand for a positive conclusion. If at the secondary operation a Jackson's membrane or Lane kink is present, while at the original one there was none, it will be evident that the condition itself is not congenital, whatever the underlying causes may be; and, further, the relative frequency of these conditions in children as compared with adults would be enlightening. But a sufficient material can hardly be obtained except by the co-operation of a considerable number of men.

Dr. Connell (closing the discussion on his part): I aimed in my paper to emphasize the importance of the etiology, because rational treatment rests absolutely with the etiology. If these conditions are secondary to inflammatory changes in the neighborhood, either within or without the lumen of the viscera, an entirely different line of treatment will be indicated from that which will be necessary if they are due to abnormal or defective development.

With reference to plication of the dilated cecum or colon, a method of treatment mentioned in the discussion: In all such cases that I have seen there has been a plainly demonstrable obstruction, either by a kink at the hepatic flexure or by an artificial membrane extending from the wall of the colon to the parietal peritoneum. To plicate these cases would be equivalent to plicating a dilated bladder for the obstruction of an enlarged prostate.

Dr. Eastman (closing the discussion): I do not believe that any good can come out of guessing or opining as to the origin of Jackson's membrane. If any one will examine the abdomen of twenty fetuses after the sixth month, he will have the opinion that I have, namely, that Jackson's membrane is a persisting fetal fold of Jonnesco, a definitely described peritoneal fold, which passes from the mural peritoneum across the ascending colon and is inserted into the mesocolon on the inner side. The fossa, in which the head of the cecum and the appendix are often ensconced, is formed by the fetal fold of Treves. Furthermore, one will conclude that Lane's ileopelvic band is probably the persisting genitomesenteric fold of Reid. The picture of the genitomesenteric fold of Reid which I have shown, is almost precisely similar to that given by Dr. C. H. Mayo which illustrates the ileopelvic band of Lane.

That fusion or inflammation during fetal life or later, has some part in this process there can be no doubt, because we see unmistakable evidences of it, for example, in that condition in which the two layers of the omental peritoneum fuse with the serosa of the colon. I have seen this state of affairs in the adult, and it is described by several men as having been observed in the fetus. You will be surprised at the frequency with which you can open the abdomen of fetuses promiscuously and find Jackson's membrane, or find the caput coli and appendix in a pocket made by the bloodless fold of Treves, or find the genitomesenteric fold described by Reid, and observe how it corresponds to the ileopelvic band of Lane.

RETROCECAL APPENDICITIS

JABEZ N. JACKSON, A. M., M. D.

KANSAS CITY, MISSOURI

In the voluminous literature on appendicitis one finds but little of particular attention paid to the peculiar characteristics and sequences of inflammation of this organ when of the retrocecal or retroperitoneal type in anatomical location; and yet anatomical observations have shown that in about 20 per cent of all subjects this position of the appendix is found. We have personally had a rather large proportion of cases of retrocecal appendicitis, and have been impressed with certain features which affect the diagnosis, prognosis and treatment to such an extent that it has seemed worthy of more attention than has usually been accorded.

In the more normal anatomical type the appendix from the base of the cecum passes more or less downward or downward and inward and becomes an organ more or less associated with the large peritoneal fossæ. In consequence, when it becomes the seat of an acute inflammation we have associated a greater or less degree of diffuse peritonitis involving both the contiguous viscera of the greater cavity and the anterior parietes; and it is this associated peritonitis which is responsible for those most marked symptoms which make its picture characteristic and its diagnosis relatively easy. We today recognize the fact that in the earlier stages of appendicitis the symptoms are largely referred to the epigastrium or stomach and

are represented by clinical signs—epigastric pain and vomiting. When the infection has progressed further and reaches the peritoneal envelope these epigastric symptoms largely give way and in their stead the local pain, tenderness, and rectus rigidity resultant from peritoneal irritation or inflammation manifest themselves and become the final diagnostic signs. These signs vary in rather important ratio with the extent of peritoneum involved, and hence it is that, after all, our index to ordinary diagnosis is the attendant peritonitis rather than the appendicitis itself; and particularly is this true of the peritonitis which involves the parietal wall, for from this source we today recognize the principal symptoms of local pain and rigidity arise.

In the retrocecal type of appendix, the anatomical relation of the appendix to the general peritoneum is quite different, and in consequence the sequellæ of infection therein are in a measure quite distinctive. The term retrocecal appendix refers in general to that type of appendix which, from its origin at the base of the cecum, runs more or less directly upward or slightly outward and upward and is placed either directly behind the cecum and ascending colon or at least under cover of their overlap on the outer side. We have found indeed four rather separate anatomical subvarieties of the general post-cecal type: 1. The appendix, possessing its usual mesentery, is only distinctive in the fact that it runs upward along the outer side of the colon, which overhangs and confines it in the limited peritoneal space external to the colon. 2. Another type runs upward external to the colon under cover of the peritoneum of the posterior parietes which forms its investment usually incomplete on its posterior circumference, and even though complete not furnishing a mesentery proper. 3. Again, we have found the appendix running up along the external wall of the colon itself and invested by its proper

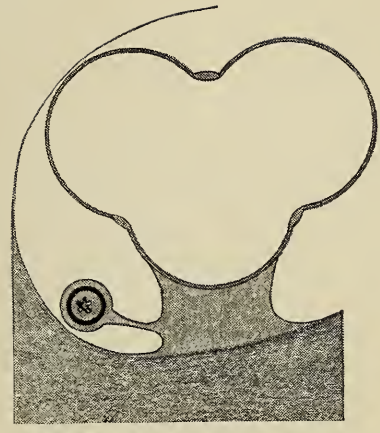

Fig. 1. Type 1 of retrocecal appendix with mesentery.

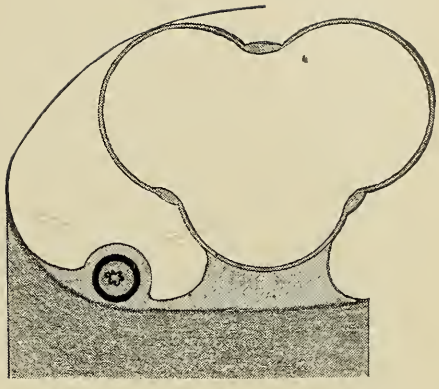

Fig. 2. Type 2 of retrocecal appendix beneath, but only partly invested by parietal peritoneum.

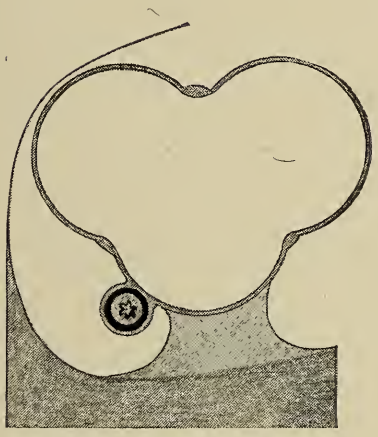

Fig. 3. Type 3 of retrocecal appendix running alongside of colon and invested in part by the peritoneum of the colon itself.

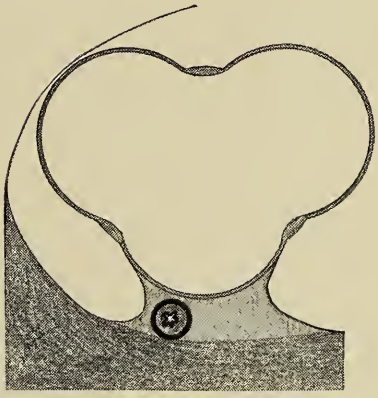

Fig. 4. Type 4 of retrocecal appendix entirely behind the colon with no peritoneal envelope.

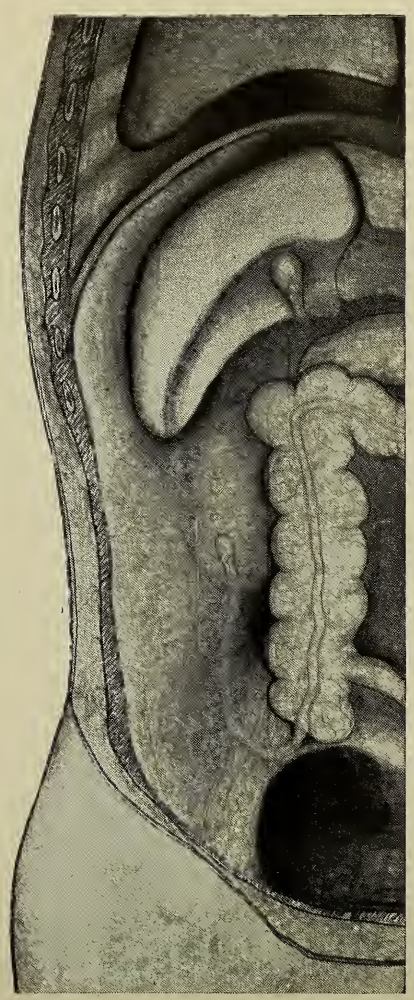

Fig. 5. Course of the more usual type of retrocecal appendix with routes of peritoneal extension obvious by exaggerated anatomic arrangement.

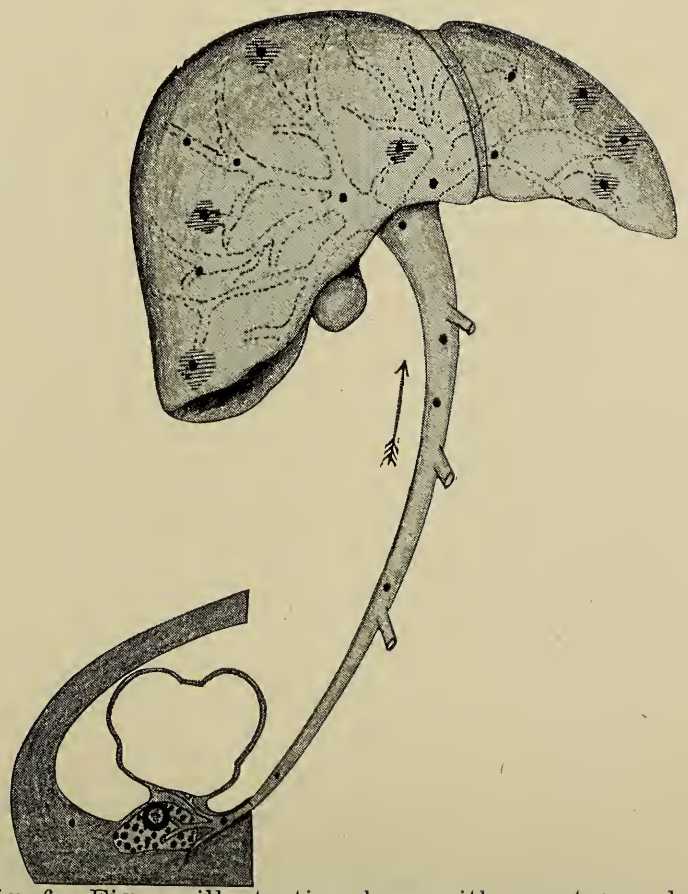

Fig. 6. Figure illustrating how with a retrocecal abscess, thrombi may pass into the portal with secondary liver abscess.

tunic and likewise without mesentery. 4. In the fourth type the appendix runs upward directly behind the colon beneath which it is buried in connective tissue entirely and has no direct peritoneal investment whatsoever.

In the first of these varieties therefore we have an intraperitoneal appendix with mesentery; in the second and third varieties we have rather a subperitoneal appendix; while in the fourth we have truly a retro- or extra-peritoneal appendix. A recognition of these anatomical types of the retrocecal appendix will prove interesting in the observation of certain sequences of infection of the organ.

In the chronic or subacute varieties of appendicitis, the retro-cecal type will present no particular significance, except that the tenderness incident thereto is situated rather far out, extends upward and backward toward the kidney and posterior under surface of the liver, ofttimes presents a particular spot of tenderness on pressure above the crest of ilium behind, and can thus ofttimes be anticipated prior to opening the abdomen. It is furthermore of interest in explaining the fact that so many cases are reported by amateur surgeons as lacking an appendix because they fail to recognize this type of appendix and do not know how to find it after they are in the abdomen. Its detection will require a good exposure of the extracolic fossa, and in the extraperitoneal variety the outer leaf of the mesocolon may have to be first incised, and the colon partially lifted up from its bed. Unless of the first type it will, when found, present no mesentery and will require enucleation instead of mesenteric ligation.

In the more acute infections certain other characteristics may be worthy of consideration. In the first place, in nearly all cases the attendant peritonitis is

quite restricted and is confined pretty accurately to the extracolic fossa. In fact, the appendix is so thoroughly overlapped by the colon that early adhesions ofttimes confine the peritonitis to a limited area close down at the base of the outer leaf of the mesocolon practically entirely subcolonic. In some instances where the infection is most obvious at the base, and particularly where perforation occurs at this point, we may have a leakage and extension of peritonitis downward into the pelvis, as well as backward and upward along the outer side of the colon; in fact, the presence of this lower pelvic infection may lead one to overlook the extension backward and upward and to content oneself with drainage of the pelvis alone, while a posterior infection is allowed to continue its course unrecognized, an error which led to several fatalities in our early experience.

While we have stated that the peritonitis is here restricted, we should modify this statement as intended only in so far as the involvement of the general peritoneal cavity is concerned. As a matter of prime importance we must recognize that in this restricted area outside of the colon a very important, possibly extensive and dangerous, extension of peritoneal infection may be going on ofttimes unappreciated and unrecognized because of its somewhat hidden site. This is the upward extension of peritonitis along the outer side of the colon into the loin and onward toward the under surface of the liver. Reaching here it may follow around, now forward above the hepatic flexure of the colon beneath the liver and result in a subhepatic abscess or further invade more or less extensively the upper peritoneal cavity beneath the liver. More usually, following gravity, it reaches the low fossa in behind the liver, passes upward between the liver and diaphragm, and results in a subphrenic peritonitis, ofttimes terminating in an obscure subphrenic

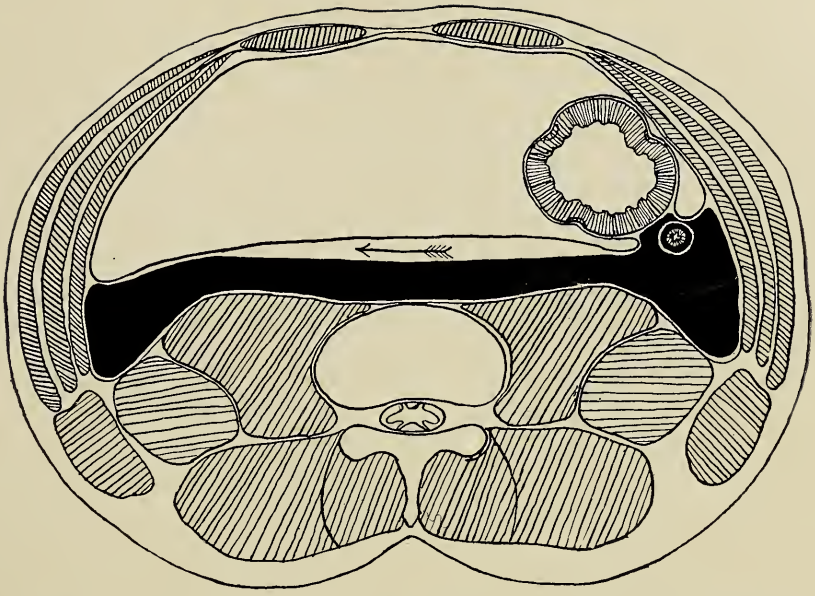

Fig. 7. The black shading shows how pus may pass behind the peritoneum across the median line to the left lumbar fossa.

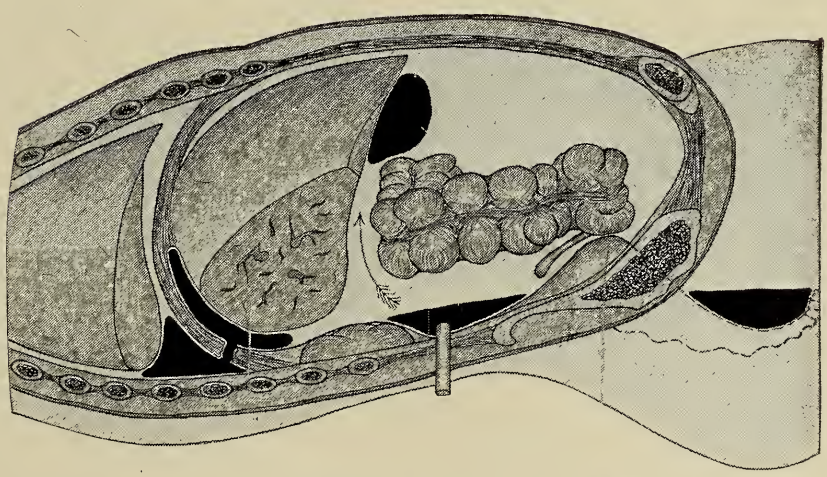

Fig. 8. Shaded areas illustrate fossæ of pus, the route of extension and the importance of the posterior drainage in recumbent patient.

abscess. From here the infection may pass through the diaphragm into the pleura with resultant empyema of this cavity, or in the event of lung adhesions it may break into the lung itself and produce a lung abscess, or rupture into a bronchus and be thus evacuated with a possible septic bronchitis added. In fact, in one of our cases we had a rapid death from rupture of such an abscess into a bronchus, coughed up and inspired with virtual drowning of the unfortunate child. Surely, with these possibilities of lumbar abscess, subhepatic abscess, or peritonitis, subphrenic abscess, empyema thoracis, pulmonary abscess, and septic bronchitis or pneumonia, Deaver was justified in the statement attributed to him that "the retrocecal appendicitis is a bad type of appendicitis."

But outside of peritonitis and its extension there are further factors of peculiar importance. In all save the first type of our classification, it will be observed that the appendix is in some part at least extraperitoneal or retroperitoneal. Infection may therefore spread to the cellular and other retroperitoneal tissues, as well as to the peritoneum. A cellulitis is therefore one of the further dangers and may occur with or without a coincident peritonitis. This cellulitis may become localized or diffuse, may extend by continuity of the retroperitoneal space, or may be diffused through the subperitoneal lymphatics. Also, as a matter of distinctive significance, the infection in this retroperitoneal space may extend to involve the blood-vessels, particularly the veins, and give rise to infective phlebitis with dissemination to different parts of the body and more particularly to the liver.

By direct extension through the retroperitoneal connective tissue, it is easy to imagine how extensive the infection may become, and, should the patient survive the general sepsis sufficiently long, how varied may be the points of presentation of the ensuing ab-

scess. In one of our cases the entire perinephritic space was involved to such an extent that on posterior incision for drainage the kidney almost floated out of the wound. Many cases of perinephritic abscess may thus be explained. In this case of ours just mentioned, the posterior drainage instituted did not suffice to prevent death, and on autopsy it was found that the infection passing behind the peritoneum had crossed the spine to the opposite side, where an equally extensive abscess had been left unrecognized. In another case the abscess had occurred in the psoas magnus muscle which had been largely destroyed with an abscess extending from the pelvic brim well up toward the diaphragm. In another case the infection spread through the parietal wall posteriorly and laterally and then followed down the thigh on its anterior and outer aspect to the knee.

These few instances are sufficient to illustrate the multiform direction of extension as met with in our own limited experience and might be almost indefinitely extended if recourse were made to general literature.

The possibilities of extension by the portal radicals is also of importance. We have had the unfortunate privilege of making an autopsy on two of our own cases with resultant liver abscesses, medically and surgically hopeless, resultant from a small retroperitoneal abscess unrecognized in time for any hope from surgical intervention.

This hasty and brief sketch limited to personal experience is sufficient, we think, to illustrate the importance of special study of the particular features of retrocecal appendicitis, which, in our judgment, more than any other type, requires early diagnosis and prompt, and in some respects, special, surgical attention.

Symptoms.—From what has been said about the special anatomical relations of the retrocecal appendix we are prepared to appreciate some of the peculiar features of symptomatology and the necessity ofttimes of very close study to arrive at a correct early diagnosis. In our judgment the difficulty of diagnosis, particularly in the hands of the general practitioner into whose care all cases first come, and the consequent failure to secure prompt or proper surgical care, are responsible for the designation of Deaver, "the bad appendix." And it is particularly the absence of a diffuse peritonitis which leads to the failures of diagnosis, since, as we noted in our introduction, it is upon the distinctive features of peritonitis, and particularly of parietal peritonitis, that we have mainly relied for diagnosis. In retrocecal appendicitis these symptoms may all be largely lacking or, if present at the onset, may quickly disappear. We have thus frequently had our medical confrères remark: "We thought at the onset that we had a case of appendicitis, but the symptoms of this quickly disappeared, and since we have been at a loss to know what was the trouble." This remark serves to illustrate the important significance of the early symptoms and the general failure to appreciate the transition to those peculiar to the retrocecal type.

We shall here refer to only the acute variety of appendicitis, as the subacute or chronic present no clinical peculiarities other than before mentioned.

Epigastric Pain.—The primary epigastric pain is usually marked in this type, as in others of acute appendicitis.

Vomiting.—Vomiting usually accompanies or follows the epigastric pain, as in the ordinary case.

Local Pain and Tenderness.—In the usual case, the epigastric pain and vomiting is followed in a short

time by a transfer of pain of acute type to the lower abdomen and perhaps to the region of the appendix in particular, and is attended by a marked tenderness, either local or diffuse, over the lower abdomen in general. This lower abdominal pain and tenderness is not only marked but persistent, unless sudden perforation supervenes. These symptoms are, in large measure, the indication of approaching or present more or less diffuse peritoneal involvement. And here the divergence in symptoms begins. In the retrocecal type, there is usually some pain and tenderness *early* in the region of the appendix. This pain, however, is not usually so severe, and the tenderness is much more circumscribed. Then, as the causative peritonitis is usually quickly localized under and behind the outer side of the colon, with exceedingly slight or no involvment of the anterior parietal peritoneum, the pain becomes either markedly decreased or may practically disappear entirely. Likewise the tenderness hidden behind the cecum becomes diminished and may be entirely missing on palpation over the abdomen in front. Pressure from behind just above the crest of the ilium and in the loin, will almost invariably elicit, ofttimes even by the patient unsuspected, tenderness, more or less acute. And herein is presented one symptom elicited by proper examination, which is very significant indeed when associated with the primary symptoms above noted, and should be definitely borne in mind.

Abdominal Rigidity.—Abdominal or rectus rigidity, so significant in the intraperitoneal appendix, is here likewise usually of transitory presence. What rigidity remains after the first onset is limited to the lateral abdominal muscles and the rectus may become soft and negative.

Abdominal Distention.—More or less distinct ab-

dominal distention marks the normal appendicitis because of the involvement in the peritonitis of the adjacent small intestines. In the retrocecal type, on the other hand, the peritonitis is restricted to the colon, and the small intestines are not involved. The accompanying distention may therefore be entirely lacking or, if present, moderate and soft.

Tumor.—In the general type of appendicitis, particularly when leakage occurs, we soon have a distinct tumor-mass easily recognized through the anterior abdominal wall. The edema of the contiguous guts will present an apparent tumor ofttimes, even though there be little or no leakage or peritoneal exudate. With the retrocecal appendix whatever edema or leakage or exudate may be present is under cover of the overlying colon filled with gas, fluids, and feces, and ofttimes can not be recognized at all. If found at all it will be well outward and backward and ofttimes present only in the loin.

Temperature and Pulse.—The temperature elevation and pulse increase are of course present in any infection. It is noteworthy that they *persist even after the local signs apparently disappear.* This persistent temperature after subsidence of local signs has led many a case to be diagnosed as typhoid fever, tuberculosis, malaria, or one of any variety of infection characterized by fever. As a matter of fact the temperature usually remains at from 101° to 103° and is out of proportion to local signs, and the pulse is increased in corresponding septic ratio, too high for typhoid and such infections.

Blood-Examinations.—The examination of the blood is particularly serviceable here. The negative Widal may be of service in excluding typhoid, and the leucocyte count shows always a great increase in the white cells characteristic of pus-infection.

Sepsis.—The general picture of septic infection will persist and increase in spite of the apparent subsidence of recognized local signs of infection and should point the way to the recognition of at least some focus of distinct infection, only necessitating proper local assignment. But, after all is said, the recognition of the local area of infection rests on (1) the *primary symptoms,* requiring close attention to the earliest history of the case; (2) the tenderness usually found posteriorly; (3) the blood examination and other signs of pus-infection.

Treatment.—In treatment a few points of practical significance we have found worthy of special consideration based upon our own experience. It is pretty generally conceded today that in any case of acute appendicitis early operative interference is the proper procedure. In the retrocecal appendicitis we belive this to be of particular importance, first, because of the obscurities which arise with delay and, second, because of the dangerous extensions which are so apt to occur without well-marked signs to indicate their menace. If one waits until infection has spread by the continuity of peritoneum to the subphrenic space, most difficult problems of localization and management will arise. A subphrenic abscess may be very hard to locate, even when we are reasonably sure of its existence somewhere. And even though located, its access is difficult and limited in its hidden site. And possibly while we hesitate, carefully seeking the spot of infection, it is already making its way through the diaphragm into the pleura, the lung, or bronchus. We have faced just such situations ofttimes with hesitation and uncertainty fatal to the patient.

Furthermore, while we delay, in the retroperitoneal type we may permit embolic infection of the liver with multiple abscesses entirely beyond any hope

of relief by surgical means or otherwise. We would therefore emphasize the need of early diagnosis and early removal of the appendix as particularly indicated in this anatomical type of appendix.

If operation can be instituted while the trouble is still confined to the appendix there is little special to be said except that a rather larger incision is necessary to expose the appendix, which probably has no mesentery, and ofttimes the incision should be rather further back than that employed for the more usual lower site.

After perforation has occurred with peritonitis and abscess some further factors are important. In opening the abdomen from anywhere in front, as customary, we will usually enter the free peritoneum, as our abscess will be hidden down beneath the colon by adhesions. It is then well to pack off well toward the median line to prevent any general infection when we later break through the adhesions into the abscess. To avoid this condition a posterior incision in the loin has been advocated, and may in some cases be advantageous. It gives, however, little opportunity for observation and makes it rather difficult to reach and deal with the stump of the appendix at the base of the cecum. If we have a rather large abscess and are content to incise and drain it is a route to be preferred. When we expect, however, to remove the appendix at the time, as we practically always do, the anterior incision, extended backward if desired, has been our route of selection. In many cases the appendix will be found long, extending well up to the under surface of the liver, and the incision will have to be extended backward until fairly long in order to give sufficient exposure if removal of the appendix is desired.

But one point we have found of great importance. It must be remembered that the space about the kid-

ney and below the liver is at a very distinctly lower level when the patient is lying down than is the iliac fossa, where the base of our appendix is situated, and which is directly beneath our anterior incision. In drainage therefore if we merely put in drainage in front as is customary in other types of appendix, we leave a large fossa behind, which may retain a considerable pool of pus; and this pool, if pus, is just at the danger-spot for the extensions which we have cited as particularly menacing in these cases. This fact was forced upon our attention some years ago in a case upon which we had operated. The drainage, which had been put in front, was apparently effective with a profuse amount of pus; and yet our patient remained unaccountably septic. After having carefully cleaned up the wound one morning, in attempting to turn the patient over to remove the Kelly pad, our hand was pushed forward from the lumbar fossa when the clean wound immediately flooded at least a pint of pus and caused us thus to realize the inadequacy of the drainage from in front. Since this experience we *always* seek the lowest point of this lumbar fossa from in front, and make from behind a sufficiently large incision down to the bottom of this pouch to provide for a large drainage-tube nearly one inch in diameter. This tube is carried just far enough in to well drain the bottom of this area. Until we learned this one thing we had a mortality of at least 75 per cent in our cases of retrocecal appendicitis from insufficient drainage and infection, which continued to spread. Since learning this lesson and invariably employing this posterior drainage the mortality has been nil, except in cases so far neglected as to have already secured the remote infections before any surgery was instituted. A cigarette-drain is usually added in front, as in the normal case. Where the abscess is first encountered well below at the base this extended infection behind may

well be overlooked, as has occurred to us once or twice, and in each instance with death. The one surgical feature therefore for strict observation in retrocecal appendicular abscess, is posterior lumbar drainage.

DISCUSSION

Dr. W. W. Grant (Denver, Colo.): This interesting paper by Dr. Jackson is worthy of consideration by the Association. He has laid great stress upon the greater danger of infection and of subdiaphragmatic abscess in these cases. I think Reclus was the first to call attention to this form of appendicitis, and the common sequel of subdiaphragmatic abscess and infection of the liver from it.

I wish to call attention to one symptom which Dr. Jackson did not mention. In a paper read before the Colorado State Medical Association in 1903 on "Retrocecal Appendicitis," I pointed out that while not always, perhaps, but generally, the tender point was not McBurney's point but about two inches directly above on an intersecting right angle line from the umbilicus; that in these cases this will be found the tender point. While these post-colonic cases are more apt to affect the organs in the upper abdomen, it is a fact that we are not so apt to witness extravasation into the abdominal cavity and consequently peritonitis. While perforations in these cases occur just as quickly, rupture into the peritoneal cavity is not common, the perforation being in the posterior cecal space; the pus follows the course of least resistance, going upward in the region of the kidney and the liver. For this reason these cases are not so rapidly fatal without operation, yet very dangerous and a prompt operation with drainage over the lumbar region will cure as large a proportion of these abscess cases as any other form of disease.

Dr. Walter B. Dorsett (St. Louis, Mo.): I believe the late Dr. Joseph Price was the first to bring this matter before us in a paper, or at least in a discussion, in which he related a case of a young girl, seventeen years of age, who, after having been ill for some time, was treated for pneumonia, had a violent attack of coughing, and coughed up her appendix. I have noticed these cases a number of times, and I was particularly interested today in Dr. Jackson's graphic description of this condition, and mostly in regard to the necessity for lumbar drainage, even after we have opened anteriorly. Three such cases have come under my observation in which I was fortunate enough to

make a diagnosis of rectrocecal appendicitis, and opened first through the lumbar region and drained well, and then went in afterwards in front and removed the appendix, for the reason that the appendix is very hard to remove through a lumbar incision, so that I thought possibly it might be worth while to take into consideration the drainage before the ablation of the appendix.

Dr. W. T. Reynolds (Kansas City, Mo.): In the study of retrocecal appendicitis, we are amazed at the striking absence of text-book mention of this condition, and especially so when it is admitted that 20 per cent of all cases of appendicitis are retrocecal.

There is no mention of this condition in "The American Practice of Surgery," edited by Bryant and Buck, and a mere mention in "Keen's Surgery." Howard Kelly, in his classic volume devoted to appendicitis, mentions it more particularly, but says as yet no definite symptomatology has been worked out.

I think we are indebted to Dr. Jackson for his paper, and especially to the pertinent points of diagnosis and his emphasis on lumbar drainage in treatment.

THE APPLICATION OF A PROTECTED QUARANTINE PACK IN ABDOMINAL SURGERY

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PORTLAND, OREGON

The treatment of acute inflammatory and suppurative diseases and their sequelæ within the peritoneal cavity, constitutes a large part of the surgeon's work. No part of his work requires more thorough knowledge of underlying principles and more mature personal judgment.

In studying this subject we must consider, first, the means that nature uses, how she applies them, under what circumstances she works best, and what results she leaves after her cures.

When an infectious agent is introduced into the peritoneal cavity, nature attempts to surround and destroy it by using elements derived from the blood. Failing to surround it or destroy it, she attempts to absorb it and carry it out through the lymph- and blood-stream and dump it into nature's sewers (the kidneys, bowels, and skin). She surrounds the enemy by pouring out from the blood a quantity of fluid which contains leucocytes and plastic substances. The plastic substances stick all peritoneal surfaces together around the area, and the leucocytes destroy. Any agent whilch keeps the movable organs, which are covered with peritoneum, in motion, such as food and cathartics, tends to prevent the surrounding of infec-

tion by pulling the intestines over the abdomen, scattering the infection in all directions and thereby prevents nature's agents from destroying the infection. But in so doing, the infection in being distributed over a larger area may absorb all the more rapidly, bringing about a very rapid and uneventful cure, provided the infection is mild or limited in quantity, but bringing about rapid fatality if the infection is extensive or very virulent. This explains the good and the bad features of the two opposed theories of treating peritonitis, namely, cathartics and opiates, that has existed in the past. We may also explain the advantages and disadvantages of the opposing views as to postural treatment as advocated, on the one hand, by Clark, who advocated the extreme Trendelenberg position, without drainage, to promote absorption after removing all he could by sponging and washing and leaving salt solution in the abdomen. The second theory was advocated by Fowler, who used the reversed Trendelenberg position with drainage.

Both were right, for Clark was dealing largely with the low grade of infection found in chronic pus-tubes, while Fowler was dealing chiefly with the very virulent infection of acute fulminating appendicitis.

The same differences have arisen as to the material and quantity of drainage to be used under various circumstances. Suffice it to say that there are undoubtedly times when a small tube should be used; also times for a large tube, a small gauze and a large gauze, etc., but this will not be discussed except in so far as it is incidentally mentioned in giving the indications for the use of the *quarantine pack.*

Concerning the formation of adhesions: We may say that some patients are particularly prone to the formation of adhesions, probably due to some condition of the blood. When an inflammatory process or trau-

matic influence is sufficient to cause an outpour of serum, and yet not sufficient to prove fatal or form an abscess, the watery elements of the exudate are absorbed while the plastic elements remain as a residue and form adhesions.

When intestines are exposed to the air or to traumatism for a considerable length of time or severity, serum is poured out, and adhesions result.

When a slight infection is present and is cared for by nature, serum is poured out, and adhesions result.

When a drain is insufficient in size to deliver the serum to the surface as fast as it forms, it is soon blocked, serum accumulates, and adhesions form. A drain poked at random into the abdomen sometimes holds an intestine against another organ and produces a very firm adhesion and at times actual peritoneal blending. When a drain is large and the serum is delivered rapidly to the surface, adhesions are not extensive, as a rule. Extensive adhesions when broken up tend to reform unless the raw surfaces are kept apart till primary healing takes place or a granulating area is formed between.

Intra-abdominal adhesions do no harm, as a rule, unless they impede the action of a movable organ, such as an intestine. Dangerous adhesions producing intestinal obstruction are usually found in a narrowed cavity and particularly the pelvis or in the neighborhood of drainage which has been carelessly placed or is insufficient.

In using abdominal drainage we must aim at two things:

1. To avoid impending danger and save life.

2. Guard against future dangers, resulting from subsequent adhesions.

To this we wish to add a third use for drainage, namely, the cure of adhesions after they have formed.

In dismissing the general subject of drainage before applying our subject as stated, I will state that, in my opinion, the large tube is the best form of drainage for diffuse general peritonitis. A small tube or cigarette-drain is sufficient after a small focus of infection has been removed, such as an unperforated gangrenous appendix.

In the following conditions I have had better results from the use of the protected quarantine pack, made of gauze, from which the intestines are protected by rubber tissue:

1. When an infected or gangrenous organ or area cannot be, or is not, removed for various reasons.

2. When an infected or raw area is located in a narrowed or limited area, particularly the pelvis.

Gauze is the ideal material for such a purpose, first, because it drains up hill and drains rapidly if used in sufficient quantity; and, secondly, because it forms sufficient bulk to hold the intestines away from the infected or raw field or out of the pelvic cavity, as the case may be.

The principles of gauze-drainage are shown by the following experimental study:

By experiment it is found that ten gauze wicks will drain exactly ten times as fast as one over the edge of a basin or out of a cavity; therefore drainage will be in exact proportion to the amount of gauze passing through the external wound. In draining water over the edge of a basin the flow ceases when the water reaches the level of the outside end of the gauze, but starts again if a piece of gauze is pinned to the end of the drain and flows with a rapidity proportionate

to the length of the external gauze below the level of the fluid in the basin, thus acting on the principle of a siphon. Gauze drainage must therefore be brought in contact with dressings or other absorbent substance if drainage is to be effectual. The same applies when the gauze is encased, in that the end of the gauze must be in contact with dressings.

My findings, both experimentally and clinically, confirm the conclusion of Yates and others, that gauze will not drain pus or blood from a basin nor from a closed abscess cavity. It will, however, drain either pus or blood from the free peritoneal cavity. Even coagulated blood or thick pus is liquefied by the excessive flow of serum (noted by Yates as being useless and depleting) and is delivered to the surface in the form of a thin, red, non-coagulable fluid in the case of blood, and a yellow fluid in the case of pus, which will frequently saturate the dressings and all the bedding in a short time, if the drainage is sufficient in quantity and properly placed.

Thus this seemingly useless flow of fluid serves the purpose of a solvent, and at the same time acts as an irrigator from within, out, in a manner impossible by artificial irrigation. Gauze packed in a cavity like the "folds of a fan," as described by Clark, creates a flow of serum in proportion to the amount of gauze in the abdominal cavity, but serum is delivered to the surface only in proportion to the size of the strip passing out through the wound. Hence the large secretion of serum and, later, of pus, surrounding the drain (mentioned by Clark), which was not delivered because of the insufficient drainage at the outlet and which later produced a chronic fistula because the internal cavity was larger than its outlet. This is aptly illustrated by the formation of a fistula following the improper opening and subsequent treatment of an ischiorectal abscess.

Gauze forms a splendid frame-work in the formation of a protecting wall and an effective drain when in large enough quantities to prevent its being choked by granulations from the sides of the wound. Any attempt to remove gauze prior to the fifth or sixth day is dangerous, as shown by Clark, who reported eight out of one hundred cases in which intestines were drawn out through the wound and others in which walls were broken down so that infection occurred. Force should never be used in the removal of gauze, as it will almost always be loosened from the sixth to the fourteenth day by natural processes. By this time a definite wall is formed, and there is no danger of breaking open the cavity. Much of this difficulty is relieved by covering the gauze with protective tissue.

The protective quarantine pack is placed and removed as follows:

All the intestines must be temporarily packed far away from the infected or denuded area or organ. Gauze wicks the size of a finger, should be laid straight, side by side, reaching entirely across the abdomen and coming out through the drainage-opening. Four or five layers of gutta-percha tissue are then placed above the gauze and allowed to come out through the opening also. The intestines are now allowed to come down against the rubber tissue. The omentum is drawn down and, if long enough, is interposed between the rubber tissue and intestines.

In our experience it has been better to attempt to remove half or all of the gauze wicks about the sixth or seventh day after it is placed. If the gauze is removed before the end of six days, it is usually done with difficulty. If it is allowed to remain more than ten days it again attaches itself and is removed with great difficulty, so at the end of about six days it will

usually be found that the wicks are pulled out easier than they are either sooner or later. At times we remove all the gauze at this date, and at other times we leave about one-third of it and take it out about three days later. We rarely remove the rubber tissue earlier than ten days or later than fourteen days. At this time we remove all the rubber tissue, and slip in a small rubber tube, tapered at the end, which prevents the abdomen from closing over the cavity. This is allowed to remain a week or ten days, until the bottom of the opening has time to close in around the tube and make a straight sinus. If this last tube is not allowed to remain long enough, the wound heals on the outside and leaves a large unhealed cavity on the inside, which makes the wound very much slower in closing.

About five weeks is the average time for a wound thus treated to entirely heal, and it is probable that from ten to twenty per cent of these patients will later develop a hernia, which is the only drawback. We usually keep the patients in bed for about four weeks.

For the past ten years I have used the pack with splendid results and entire freedom from troublesome adhesions. I have used it a great deal in acute gall-bladder conditions and in selected cases of acute appendicitis; in all cases of extensive pelvic adhesions where much of the peritoneum is destroyed, I have used it with universal success; in more than two hundred pus-tube cases, including a number of cases of acute gonorrheal pus tubes, within the past six years, without fatality; and in a considerable number of acute infections following criminal abortions, with better results than by other means.

DISCUSSION

Dr. H. G. Wetherill (Denver, Colo.): It is with much hesitation that I take exception to anything Dr. Coffey suggests, for I have great respect for his opinion, but I find my views in this

instance so opposed to his that I must take exception to them. I feel that he goes backward a long way in urging the use of gauze as a drain at the present time. I have had the impression that it was generally conceded that gauze was not any good as a drain, that it answers very well as a pack to wall off infected areas, and that it acts well to arrest oozing; but that as a drain it had been abandoned.

I am also unable to understand why he should refer to serum as forming adhesions. My conception of the formation of adhesions has been that they were due to erosion or destructive inflammation of the endothelium of the peritoneum, and, as we all know, a gauze "drain" favors such erosion. If this erosion does not occur then adhesions do not occur. They do not usually occur on smooth, unbroken, unirritated peritoneal surfaces, any more than they occur on opposed, unbroken epithelial surfaces.

With reference to leaving gauze drains four or five days: I should anticipate that, if we were to do that today, as we did in the past, we should again have many fecal fistulæ. The gauze in the peritoneal cavity not only produces endothelial erosions, which favor adhesions, but, if left long enough, and particularly in tuberculous disease, it produces fecal fistulæ.

I regret that in this instance I must differ with Dr. Coffey. I had hoped that we had gotten away from the use of gauze as a drain.

His advocacy of early operations in acute infections astonishes me. Pelvic infections, particularly of Neisserian origin, are unquestionably better left alone till the acute stage is past. If so managed they may never require surgical treatment. I had assumed that this point was universally conceded.

Dr. William Jepson (Sioux City, Iowa): I had not intended to take part in this discussion. I am one of the members who find it necessary to review their opinions and be corrected.

I think Dr. Coffey intended us to understand that in the class of cases in which he uses the quarantine gauze-pack, as here described, he referred to those cases where he has diffuse infection, or somewhat diffuse, within the peritoneal cavity, particularly in the pelvis. It matters not, however, where. We all realize that when the peritoneum once becomes the source of infection it immediately reacts, which reaction is represented first by transudation of serum containing cytolytic elements and bacteriolysins, which destroy the germs present: and the amount of this serum may be represented from three to eight per cent of

the body-weight per hour. This is poured into the area which is infected, and, if it is wide, it is impossible for one or two rubber drains to properly convey that away.

I quite agree with Dr. Coffey, and it has been my practice for the last fifteen years, to use gauze in the manner described. I learned of it first from Dr. Price, and I have no reason now, I believe, as the result of my experience, to change it. In other words, I think one or two good-sized rubber tubes surrounded by gauze to drain all areas and protected by rubber tissue to prevent adhesions fastened to it, are excellent means of establishing drainage, and I think the rubber tissue should have small performations to permit the serum to pass through them and into the gauze. That constitutes one of the best kinds of drainage. Whether this will or will not prevent adhesions, I do not know. I have had very little experience in observing the effects later, but I do know that we have had to open the abdominal cavity in tuberculous peritonitis, and when we have to do this we often find the intestines are adherent, and if it is necessary to go in there later to remove a fistula or close a hernia, we have found all of the adhesions had disappeared. If you go to work and put a gauze drain indiscriminately into a peritoneal cavity, which is the seat of tubercular peritonitis, producing pressure, as will often be the case, you are likely to have ulceration, and in quite a percentage of cases you may expect a fecal fistula following.

Dr. Van Buren Knott (Sioux City, Iowa): I am not quite sure of the position taken by the essayist in regard to this matter. If I understood him correctly, I believe he advocates the use of gauze pack as a quarantine in certain cases where it is impossible to remove an organ that is very much impaired or diseased. In that I agree with him that quarantine with gauze will serve a useful purpose and prevent the spread of infection, but that gauze should be used as a drain in the abdominal cavity I do not believe, and I agree with Dr. Wetherill that this is a backward step. So far as I know and from my observation of cases and from the experience of friends of mine who are reporting their results in the treatment of peritonitis, there is absolutely no comparison today between the results procured by drainage, not with a tube the size of this, but with a real tube of the size of garden-hose, an inch in diameter.

I hear men talking about drainage-tubes which are used in the treatment of general peritonitis, and that drainage of the peritoneal cavity will not occur after about four hours. The drainage must be ample. A little tube cannot relieve that peri-

toneal fluid, so the drainage must be free, and the tube should be a large one.

I cannot agree with Dr. Coffey if I understand the pictures rightly. So far as the formation of adhesions is concerned, there is a tube which is absolutely improperly placed. That tube, according to the picture, should never have been so placed. The tube is impinging on a coil of ileum, which produces necrosis, and with a tube of this size there will be a lot of kinking of the intestine, which will occur back of the wad of gauze. According to this picture, the gauze pack is displaced from the natural position, and there is greater danger of adhesion than there would be with a drain of less diameter. But with a drain which is smaller than this, and with a smooth surface, it is not possible to protect the gauze by enveloping it with rubber tissue. There is danger that some portion of the gauze will escape and become adherent. Suppose the gauze packing does not carry off all serum, a smooth rubber tube, properly placed in the cul-de-sac at this point (indicating) will not cause as much outpouring of the serum as the pack will. Gauze in the abdominal cavity is a cork. I have removed this many a time in secondary operations for peritonitis in which gauze was introduced as a drain, but which did not drain, and upon its removal there was the escape of considerable fluid.

Dr. M. G. Seelig (St. Louis, Mo.): There are two phases of this discussion that appeal to me as of importance, one on the practical side and the other on the theoretical. I belong to that group of men who in draining the abdomen do not use a tube the size of garden-hose. My early training was received at an institution that afforded abundant opportunity for the study of peritonitis. Excellent results were obtained with small drainage-tubes. Nature is a very kind task mistress, and a great many of our patients do not get well because of the size of the drainage-tubes, but they get well in spite of our over-enthusiastic interference. It seems to me that in surgery we have been a little slow in adopting a rather abstract point of view that has been brought out so well by the brilliant surgeon Bier, who wrote a monograph a short time ago on "Teleology in Modern Medicine," expounding the philosophic doctrine of purposefulness. He shows how, in olden times, and, for that matter, even in modern times, some people were wont to consider symptoms as devils incarnate that had gotten into the body and must at all cost be driven out. Bier emphasized the fact that symptoms are often beneficently purposeful, and that, for example, heat, swelling, redness, and pain are manifestations of a

curative effort on the part of Nature. I would like to ask Dr. Knott and others if it is necessarily essential to drain away the serum, which has been referred to as bacteriolytic and germicidal the minute it is exuded; whether it is not put there for a purpose, and, if we put in a hose-drain to get it out, whether we are actually following the line of action marked out for us by Nature. Personally, I feel in a great many instances that our enthusiasm leads us to be over-active.

Secondly, speaking for myself, I will not under any circumstances, except where the indications are distinctly marked, use a rigid rubber tube, and I mean by that, one whose wall is one-sixteenth of an inch thick, for drainage. Many of you have doubtless read the excellent report of Dr. Moschcowitz in New York, detailing the use of two rubber-tube drains after a bilateral-ureteral stone-operation, and how necrosis of both of the iliac arteries resulted. You have heard mention made of post-operative fecal fistulæ. Such fistulæ are often dependent upon one thing, the vermiform movement forcing the intestines against a rigid body. Gauze, wrapped in rubber tissue, is a flexible, non-resistant body that drains efficiently, so I cannot for the life of me see the use of sticking into the peritoneal cavity a more or less rigid tube, knowing as we do that there is a certain amount of inherent danger and harmfulness to the organism from such a procedure.

Dr. Charles H. Mayo (Rochester, Minn.): Years ago gauze was in common use for drains, but it has gradually fallen into disfavor. Dr. Knott is quite right about its acting as a cork. The pack of gauze was used to avoid the danger of hernia. A little of it was brought out of a small opening between compression or tight sutures. In this way the drainage was corked up, so to speak, and created the irritation referred to.

Dr. Coffey spoke of septic conditions and old chronic pus cases, in which the patients themselves take care of a great deal of the pus. He is right in using the gauze to quarantine the abdomen in acute conditions, but not in others. Dipping the gauze in hot paraffin or liquid alboline relieves one of the necessity of using rubber tissue. In using the latter it is best to bring it all the way around instead of to one side, thus determining the amount of traction used on the gauze in its removal. Gauze should never be left in contact with the sutured intestine, for it may become incorporated in the process of healing and upon removal will cause a fistula. In the general peritoneal cavity gauze loosely applied drains an enormous amount of ma-

terial in twenty-four hours. We use it frequently around the gall-bladder.

Dr. E. P. Quain (Bismarck, N. D.): I have used rubber for some time for the purpose of walling off the intestines in abdominal sections. Dental rubber is the material used. The pads or sheets are prepared by fastening a few layers of gauze to one side of the rubber by means of a few mattress stitches. The gauze pad should extend a half inch or so outside the rubber margin, in order to act as an anchor against the parietal peritoneum. If this is not done, loops of intestine will slip through between the rubber and serosa and enter the operative field. Several-sized pads are convenient. Damage to the intestinal serosa is reduced to a minimum by this method.

For the purpose of surrounding gauze-drainage on all sides with rubber protection I have found a convenient means in a rubber glove with the fingers cut off. It forms a tube through which gauze or wicks may be packed as tightly as desired. Several such glove-drains may be placed side by side if needed.

As Dr. Coffey pointed out, there can be no question that gauze will drain fluids out of a cavity; but it is necessary either that the outer end of the drain be lower than the cavity or that a sufficient amount of dry gauze be applied on the outside to absorb the fluid as rapidly as it comes to the surface. When the dressings are saturated, drainage stops. The practical hint is, that the wound should be dressed frequently.

Dr. Coffey (closing the discussion): As I sat listening to some of these papers and trying to follow the authors, I could not help but think what a poor listener I am, and from what has been said I think Dr. Wetherill is in my class, because I specifically stated the cases in which I would use this method of quarantining. Dr. Mayo has stated my position exactly on this subject. Since the subject has turned very largely on the question of drainage, I will state as my opinion that the large tube is the best form of drainage for a diffuse general peritonitis; and that a small tube or a cigarette-drain is sufficient for a small focus of infection which has been removed, such as an imperforate gangrenous appendix.

Where an infected gangrenous organ is allowed to remain or is not removed for various reasons, such as we find in operating upon acute gonorrheal infection, in which the uterus itself is infected, and you can see the red area and pathologic condition of the tube, by drainage after the plan I have suggested you save the patient a long period of suffering which

goes on, because gonorrheal pus-tubes are destroyed, and after they have once become infected it is questionable in my mind whether they ever functionate again. The importance of removing the infection before the ovaries are destroyed is much greater than trying to save the tubes. We can do this by attacking the acute infection. When there is a so-called lymph-space or area that is raw, which you cannot cover by peritoneum, gauze serves a useful purpose.

I will have to send Dr. Wetherill an article on the principles of drainage, because it seems he has been devoting his attention to one particular thing, and that is the tube. There are a good many principles involved in drainage in which the tube is absolutely essential, and there are those cases in which the gauze pack is essential. A small tube, such as that which has been mentioned here, is of no value. It does not drain. It does not do much of anything. It is in the way. I have used the tube myself in those cases, and intestinal obstruction has occurred afterward. We have to quarantine with gauze in severe conditions and in acute conditions, and we have not had a case of intestinal obstruction in the course of several hundred cases because the intestines are packed out of the way, while the drain is being placed. There is no chance for a knuckle of intestine to adhere or become kinked while the acute process is going on as there is rubber tissue between. The intestines are held back, and we take the gauze out gradually, and the plan is to allow granulation to take place around it so that primary healing will not take place. When you see these patients afterwards adhesions are absent.

With reference to the matter of fecal fistula: If you put in gauze protected with rubber tissue to pack the intestines out of the pelvis, I would like to know how you are going to get a fecal fistula. We have not had to deal with a single case of fecal fistula with this form of drainage, but we have had several cases of fecal fistula where we have used tubes. We use this method of quarantining with gauze in acute pelvic conditions which cannot be removed and for the relief of adhesions which cannot be remedied in any other way and cannot be covered up with peritoneum.

GUNSHOT WOUNDS OF THE ABDOMEN

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The greater relative frequency of gunshot wounds of the abdomen in civil practice than occurs in military practice, is explicable by the closer range and by the more accurate aim of the assailant in civil life. Younge (*Journal Royal Army Medical Corps,* Aug., 1904) reports 1,053 rifle ball wounds occurring in the Boer war of which 92 (9 per cent) were of the head, 85 (8 per cent) of the chest and 66 (6 per cent) of the abdomen. My own experience, limited to civil practice, consists of 41 cases, of which 13 (31 per cent) were of the head, 8 (19 per cent) of the chest, and 13 (31 per cent) of the abdomen.

The wound of entry is usually to be found in the anterior or lateral aspect of the abdomen, though the bullet may enter from the back; and not infrequently bullet wounds of the chest also penetrate the abdomen. Two cases in this series were shot in the back, one dying of hemorrhage before reaching the hospital, the result of damage done by a .44-caliber bullet. Three cases shot in the chest also involved the abdomen. Blundering cases of abdominal involvement when the wound of entry shows in the thorax, may be avoided by ascertaining the approximate angle at which the bullet entered, the posture of the victim at the time the shot was fired, and by bearing in mind the anatomy of the region likely traversed.

A boy, aged 11, while on the ground, was homicidal-

ly shot by a companion with a .22-caliber rifle, from the top of a fifteen-foot barn at a distance of about seventy feet, thus giving a marked downward course to the projectile. The wound of entry was in the 7th left interspace in the mammary line. There was no wound of exit. This patient was seen in consultation thirty-one hours post-trauma, at which time he had a pulse of 140; axillary temperature, 102.4°; costal respiration, 60; a greatly distended and tender abdomen; dullness in both flanks; liver dullness elevated; leaky skin; hippocratic facies and constant vomiting. Enemas had brought some blood. Operation was, of course, not undertaken, and he died forty-eight hours post-trauma. An autopsy was not permitted.

A greater number of perforations may be expected if the bullet traverses the abdomen transversely or obliquely than if its direction be anteroposteriorly.

The more one sees of shot cases the less dogmatic does one become in asserting that any given portion of the body is not involved, irrespective of its remoteness from the wound of entry. Distant organs not even suggested by the wound, the history of the accident, or by the early symptoms, sometimes lodge a bullet. Such a case I well remember because it proved so chagrining. It is worthy of report, not only as illustrative of the preceding statement, but also on account of its rarity. A boy, aged 10, was accidentally shot with a .22-caliber rifle by his brother at a distance of about one hundred feet. They were walking on level ground. *The bullet entered the external aspect of the right thigh at its middle third.* There was no wound of exit. The wound was cleansed and dressed, and the parents told to bring the boy to the office in a few days. On the third day I was summoned as the boy complained of intense abdominal pain and inability to defecate. Imagine my surprise when on arrival

the boy was found dying of peritonitis. Death occurred eighty-two hours post-trauma. It would have been of extreme interest to have ascertained the path of the bullet, but as an autopsy was not allowed the path by which it gained entry to the abdomen will remain forever unknown. Had the patient been in the hospital, the early signs of abdominal involvement would likely have been noted and the boy given at least a chance for his life; whereas, his location in an adjoining town without care merely emphasizes the importance of having all cases under trained supervision.

After entering the abdomen bullets traverse that cavity in the straight line of their projection. The posture of the victim at the time of accident and the mobility and contractility of certain viscera will explain the manner in which organs are wounded which lie outside the apparent line of transit when the patient is supine on the operating-table. Though the theory is held by some men of repute, I am unwilling to concede that the soft tissues occupying the abdominal cavity are capable of deflecting a bullet.

It is universally asserted that, by reason of the comparative innocuousness of the gastric and upper intestinal contents as contrasted with the bacteria-laden contents of the lower bowel, wounds of the upper abdomen are attended with a lower mortality than are those of the lower abdomen. On account of the abundant lymph-channels in the upper abdomen, thus permitting very speedy absorption, as is evidenced by the rapidity with which death succeeds the unoperated gastric-ulcer perforations and other infective processes of the upper abdomen, and because of the considerable chance of profuse hemorrhage from the liver, spleen, kidneys, or large vascular trunks, I am convinced that this statement is without substantial foundation. The following case-report is pertinent:

A man, aged 51, was homicidally shot shortly after dinner, with a .32-caliber, soft-nosed, steel-jacketed bullet at a distance of a few feet. The wound of entry was two inches above and a half inch to the right of the umbilicus. The wound of exit was a half inch anterior to the left posterior axillary line and two and a fourth inches above the iliac crest. He was operated on four hours post-trauma. A medium incision disclosed the abdomen filled with blood, stomach-contents, and feces. There appeared a two-inch lacerated wound of the lower border of the liver, which was bleeding but slightly; 1 wound of the small intestine; 2 stomach wounds, one anterior and one posterior admitting four fingers; 3 in the colon; and 8 in the mesentery. Extreme laceration with ragged edges characterized all wounds and was doubtless due to the explosive effect of the high velocity bullet, which has a penetrating power of seven inches of solid pine. After repair and careful sponging, suitable drainage was provided. The pulse and temperature steadily rose till death occurred, eighteen hours after the operation. The autopsy showed all sutures holding, the hollow viscera were air tight, there was no fluid in the abdomen, and no macroscopic evidence of peritonitis. It was considered that death was due to toxemia, and that the absorption had been so rapid as not to have permitted the development of the gross lesions of peritonitis usually encountered.

Army surgeons seem to have shown statistically that a greater number of gunshot wounds of the abdomen in war recover under non-operative treatment than under operative treatment. Several factors which do not obtain to so great an extent in civil life, contribute to the high mortality of operated cases in war, such as (a) the long time taken to transport the injured, thus making for late operations; (b) the various and often jolting means of transport; and (c) the

poor conditions frequently surrounding the operation, etc. Moreover, I feel that their statistics are delusive, for, doubtless, many cases die on the battlefield and during transportation that do not adorn the statistics.

Penetration is not always accompanied by visceral injury, as is evidenced by the reports of several cases in the literature, and by the following case of my own:

A man, aged 32, was shot with a .38-caliber bullet in the abdomen two inches to the left and one inch above the umbilicus. The wound of exit was two inches to the left of the spine on a level with the anterior wound. Operation was refused, orders wantonly disobeyed, and yet a symptomless recovery ensued.

Nevertheless in every case of penetration of the abdomen by a bullet, multiple perforation of the viscera is to be assumed, and prompt surgical exploration is demanded.

The symptoms usually mentioned are not the symptoms of perforation, but are signs of hemorrhage or peritonitis and in the ordinary case are not to be awaited. By the ordinary case is meant one which shows a wound of entry to the abdomen. The occasions when there is apt to be difficulty in deciding the treatment, will arise from an uncertain diagnosis, and when the wound of entry is so placed as not to indicate clearly whether or not the abdominal cavity has been invaded. For instance, gunshot wounds of the back or thorax without a wound of exit may perforate any of the organs of the abdomen. In such a case one would need rely on symptoms, the more so if the history of the case were defective so one could not reasonably be led to any deduction. Hemorrhosis, hematuria, hematemesis, tympany, gas in the abdominal cavity, shock, rapid pulse, pain, muscular rigidity,

etc., are then signs of the greatest significance, particularly the pain and rigidity. Given such a case with indications of *probable* involvement of the abdomen, do not wait for symptoms, which, as previously mentioned, signify either peritonitis or hemorrhage, but proceed at once with the operation.

The site of the incision may occasionally, if the wounds of entry and of exit are both at one side, be placed laterally, though usually, in order that the entire intestinal tract may the more easily be examined, the median incision is indicated.

Needlessly long incisions, evisceration, rough handling of the gut, and long exposure are to be condemned. However, much time may be wasted by attempting a thorough search through an insufficient aperture; therefore, I always have an opening sufficiently large to permit rapid work, regardless of its length. Also, while disapproving unnecessary evisceration, yet in the presence of a belly full of blood, I do not hesitate to turn out intestines abundantly, to expedite finding the source of the hemorrhage, which I think should first be checked if profuse, and the intestinal perforations, from which contents may effuse, attended to after the bleeding has been controlled. This is at variance with the advice often promulgated, some maintaining that while searching for the bleeding point more intestinal contents may be expelled by manipulation, thus enhancing the danger of infection. I am convinced that this is an exaggerated danger, for, if the bowels are treated with the gentleness which should characterize their handling, then the tonically contracted intestine remains in statu quo, which is nature's own device to prevent expression of contents. Moreover, if the intestines be outside the abdominal cavity, as they sometimes are, then the objection does not hold. Further, even though there were to be

some discharge from the gut openings, in the presence of a copious hemorrhage, the danger of increased peritoneal soiling is of less importance than the danger of immediate death from hemorrhage, and the method advocated is the proper one to pursue. Should there be but inconsiderable bleeding, there manifestly is no occasion to search for its origin, but repair of the intestinal wounds should be proceeded with methodically, and the bleeding will likely have ceased by the time the several wounds have been repaired. If hemorrhage is profuse one may be confident that its source is to be found elsewhere than in the hollow viscera, and the search should without delay be directed, as suggested by the line of transit, to the solid viscera, mesentery, omentum, or large vessels.

Wounds of the urinary and gall bladders permit immediate extravasation, as do large wounds of the stomach; whereas, most shot wounds of the intestine will show little or no extravasation during the early hours following injury by reason of the tonic contraction.

Wounds of the liver and spleen are dangerous, primarily, on account of hemorrhage. Hepatic bleeding is usually controllable by catgut sutures, or rarely, tape sutures may be necessary. Hemorrhage from the spleen is more difficult to control. Packing or suture may be tried, but if these measures fail one need not hesitate to perform spleenectomy, as the results of this operation have been very satisfactory. Wounds of the kidney may, if the renal vessels have been severed or if great destruction of the kidney substance has been inflicted, require nephrectomy. Lesser wounds of the kidney and injury to the pelvis or ureter, require suturing, and in all cases efficient drainage through the loin should be provided.

Wounds of the pancreas are always accompanied by

injuries of other viscera, their management is difficult, and consequently they present a high mortality. They should be packed and provided with abundant posterior drainage, even if anterior drainage is also supplied.

Wounds of the stomach or intestine may show only a single opening, which can happen if the bullet perforates at or near the apex of a fold, though usually there are two, one of entry and one of exit; so it is a safe rule to search for an even number of holes in the hollow viscera. Should there be difficulty in finding the posterior opening in the stomach, or should there be doubt as to its existence, a simple means of determination is suggested by Martin (*Annals of Surgery,* 1907, xlv, 669): a purse-string suture is introduced about the anterior wound as for closing, and a rubber tube inserted, when the purse-string is tightened about the tube. Salt solution is then introduced into the stomach through the tube, and should a posterior opening exist the solution escaping into the lesser peritoneal cavity would readily announce the fact. As he says, the disadvantage of washing out the gastric contents, thus further contaminating the lesser sac, is outweighed by the manifest advantage of knowing whether or not there is a second opening. However, I think this expedient would be a rare requisite. The possibility of having single openings as mentioned, is well illustrated by the following case, which presented two such injuries, thus still adhering to the general rule of having an even number of perforations:

A boy, aged 13, accidentally shot with a .22-caliber rifle, was seen in consultation with Dr. J. A. Jeannotte, six hours after the accident. The bullet entered an inch and a half below and one inch to the left of the umbilicus. The wound of exit was one and three-

fourths inches below and one inch posterior to the posterior-superior spine of the ilium and one and a fourth inches to the right of the median line. He presented all the signs of internal hemorrhage, with a pulse of 140. He was operated on seven hours post-trauma. A long median incision disclosed a belly full of blood and a small amount of semifluid feces. Most of the small intestine was hastily rolled out, the blood scooped out with the hands, and the abdomen sponged so as to discover the source of the hemorrhage which was found to be three perforations of the mesentery. These were sewed up, and afterward fourteen perforations of the small intestine, two of which were singles, i. e., the bullet had caught the apex of a fold and made a rather long opening as compared with the others in two different portions of the gut. The intestine being outside was washed clean of feces before being returned. The abdominal cavity was sponged dry, and the incision sutured, leaving an opening at the lower angle from which a drain to the pelvis protruded. The Fowler position was maintained for three days. Recovery was slow, but uncomplicated.

Trimming the bruised edges of intestinal wounds, as advised by some, I deem unnecessary, as it requires time which may be of great import. Moreover, these edges are easily infolded, and, if reasonable care be exercised that the stitches penetrate healthy tissue, there need be no feeling of insecurity. A word of caution may not be amiss: As previously mentioned, in all shot cases of the intestine seen early, the gut will be found in a state of spasticity, resembling in size the small gut of a dog. It is important therefore to be careful in suturing the openings that not too much tissue be infolded, as the bite of the suture is apt to be much deeper in the contracted bowel than the inexperienced intends or realizes. Bear this fact in

mind, and the exercise of ordinary caution will guard against this danger.

It would seem hardly necessary to insist on thorough exploration of all the abdominal viscera, but be it known that not a few cases are on record which have been lost by overlooking a distant perforation which resulted in a fatal peritonitis. It is well also to look for more than one bullet wound, as a second one will occasionally be found. In some pistol fights the combatants mean to kill, more than one shot is fired and not infrequently two bullets find their target.

Whether or not to drain should be determined by the general principles which govern drainage in all abdominal conditions. If there has been but little peritoneal soiling and there is no oozing, I think it is better to close up tight. If there is considerable oozing, or if there has been extensive peritoneal soiling, then drainage is indicated. If in doubt it is safer to drain.

As stated in the beginning, there were 13 cases in this series, 7 were operated upon and 6 were not. With the exception of the one who refused operation and recovered—a hundred-to-one hazard—the cases not operated on either died of hemorrhage before they could be removed to the hospital or were moribund when first seen. Ten cases involved only the abdomen, and 3 involved both the thorax and abdomen. These three all died. Only one was operated on when the diaphragmatic, stomach, and mesenteric wounds were repaired. The patient died of secondary pulmonary hemorrhage twenty hours after the operation. Excluding this case, which I think we may properly do, as it involved both the thorax and abdomen, and because the thoracic injury was alone responsible for death, leaves 6 cases of gun-shot wound of the abdomen with intestinal perforations, all operated on within eight hours of the accident, with one death, a mortality of 17 per cent.

Dr. W. W. Grant (Denver, Colo.): Dr. Kahn has presented a most interesting paper on strictly emergency surgery. He has done it with a great deal of skill and judgment. Judgment in knowing when not to operate as well as when to do so, is often the highest test of a surgeon's skill. It is generally men who receive these abdominal wounds, but when a man who has an empty stomach and a reasonably empty intestine, receives a stab or bullet wound involving the hollow viscera, he is very fortunate, for a distended stomach and distended intestines, at the same time, insure extravasation of the contents; and in the Boer war, to which allusion has been made, it was the judgment of the army surgeons that many of the men with such wounds neglected on the field of battle, suffering from thirst and hunger, fared better in the ultimate result, and it was due manifestly to the fact that the intestines and stomach were empty and peristalsis was absent. The less extravasation the better the result.

In reference to the diagnosis: If we know the point of entrance of the bullet and its exit, we can tell with some degree of accuracy as to the course it has pursued. But often there is no exit; for the bullet has lodged in the tissues of the body. Under these circumstances and conditions, some of which cases were illustrated in Dr. Kahn's paper, I would suggest that the *x*-ray will locate the bullet, and knowing its entrance, even if we do not know the exit, we know pretty well what course it has pursued, and this can often be done very promptly, or within an hour or two. It is a fact that every hour after about four hours increases the mortality of these wounds without operation. The mortality suddenly increases from that time onward; and therefore an operation in every case of wound from a bullet should be instituted as promptly as possible. If there has been no wound of the intestine or stomach the patient will not die from an exploratory operation, and the doctor is right in stating that the first consideration is hemorrhage, not extravasation, because it is the hemorrhage chiefly, that produces shock; it is the hemorrhage, not peritonitis, that produces immediate death, so that the first consideration is to stop the hemorrhage. This can be done only by a prompt laparotomy. With the hemorrhage controlled, then, of course, the intestines can be looked after, and the abdomen irrigated with warm salt solution (I have no predudice against it in any septic condition), and then drainage. In draining these cases I would simply use the tube, but I say so with some hesitation after the recent discussion on the use of drainage with gauze in the abdominal cavity; yet I have great faith in a tube wrapped with gauze and rubber tissue

to prevent adhesions, and it is usually sufficient in these wounds, as it is in cases of perforative appendicitis.

I do not think there is anything else I care to say, except to commend in a high degree and with some warmth, this most interesting paper of Dr. Kahn. He has had a rather unusual and very interesting experience in the town of Leadville in gunshot and pistol wounds.

TWO CASES OF OBSTRUCTION OF THE BOWELS FROM UNUSUAL CAUSES, ONE FROM STRANGULATION CAUSED BY A FIBROUS BAND ACROSS A HERNIAL SAC, AND THE OTHER FROM INTUSSUSCEPTION CAUSED BY A LIPOMA OF THE CECUM

Clifford U. Collins, M. D.

PEORIA, ILLINOIS

These cases are not reported because of any interest in the technic of the surgical treatment instituted for the relief of the conditions. The interest lies in the causes of the obstructions and in the diagnostic problems involved.

Case 1.—The patient was a male, aged 32, and a bar-keeper. On July 26, 1910, at 7:00 p. m., he was taken with vomiting, diarrhea, and pain through the abdomen. He had had a right inguinal hernia for ten years, and shortly after the attack began he found that the bowel was down in the sac. He tried to reduce the hernia, but did not succeed.

On July 27th his temperature was 100.5°, and pulse 110. He was brought to the St. Francis Hospital where an effort was made to reduce the hernia, which apparently succeeded. The hernial enlargement disappeared. On July 28th his temperature was 99.5°, and pulse 92.

On July 29th he vomited several times and complained of pain in the right lower abdomen. He was tender on pressure over the right lower abdomen. His temperature was 99.5°, and pulse 100. His hernia had seemingly been reduced and should not have been causing trouble, so a diagnosis of appendicitis was made, and immediate operation urged.

It was the intention to repair the hernia if the inflammation was confined to the appendix, so the incision was made over the right inguinal canal. After the skin was incised and the fibers of the external oblique aponeurosis separated, the fibers of the internal oblique muscle were separated and the appendix found. Some free fluid escaped from the peritoneal cavity. The appendix was removed and opened, but did not show enough pathological changes to account for all the symptoms. I began to examine the ileum, and about eight inches from the cecum I found it firmly attached to the lower right abdominal wall. Further examination revealed that a portion of it was looped over a fibrous band across the internal inguinal ring. (Fig. 1.) I managed to pass the loop back from around the band. It was discolored, but the circulation was rapidly established after it was released. I succeeded in removing the sac with the fibrous band across the neck, and have preserved it. The patient made an uneventful recovery.

Case 2.—The patient was a female, aged 50, and married. She had ceased to menstruate two months before I saw her. It had been eight years since her last pregnancy. About two weeks before she came to me she began to feel sick at her stomach and have pain in the right lower abdomen, which had gradually increased. She had vomited several times each day for several days. Her bowels had moved four or five times a day for two weeks. The passages were com-

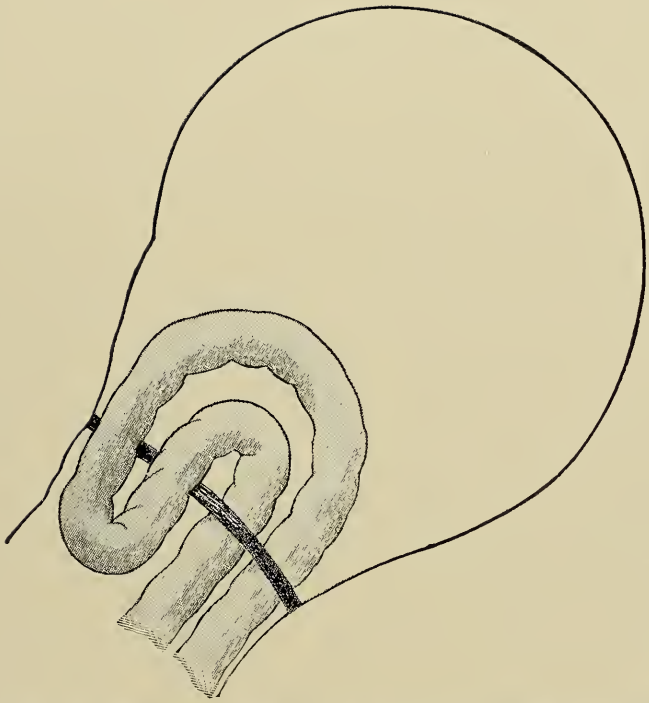

Fig. 1. Diagram of fibrous band across the neck of the sac with loop of bowel falling over it.

posed mostly of mucus and were sometimes stained with blood.

There was an oblong mass lying obliquely in the left lower abdomen. This mass was fixed. The uterus was of normal size, and was movable and seemed to have no attachment to the mass in the left lower abdomen. The abdomen was not distended. A rectal examination revealed nothing abnormal. The temperature was normal, and pulse 88.

On account of the cessation of menstruation two months before and the pain, in spite of the fact that the patient was fifty years old, a diagnosis was made of an extra-uterine pregnancy, or a possible malignant tumor of the sigmoid or descending colon.

When the abdomen was opened the colon was seen to be considerably enlarged. When the mass in the left lower abdomen was dislodged and delivered it was seen to be an intussusception. The intussusception was reduced by pressure upon the distal point. After the reduction was effected a solid round tumor, about two inches in diameter, was felt inside of the cecum. The cecum was incised, and the tumor was removed. It was attached by a pedicle to the inner wall of the cecum. It proved to be a lipoma. Evidently the tumor had started the intussusception. The patient made an uninterrupted recovery.

In *Surgery, Gynecology and Obstetrics* for August, 1909, Dr. Stetten of New York reports two cases of lipoma of the intestines and classifies seventy-five more cases found in the literature. Of these forty-one were found in the large intestine, of which six were found in the cecum. This indicates the rarity of this condition.

DISCUSSION

Dr. Carl E. Black (Jacksonville, Ill.): I did not have the privilege of reading this paper until coming here today and talking to Dr. Collins. Several weeks ago I had a case, but my

case was one of double hernia, one an indirect and the other a direct hernia, in which an effort was made to reduce the indirect hernia through the direct opening. In that case there were two sacs, and the band lying in the two sacs was a fascial division between the direct and indirect canals, so that, really, the cases are not exactly alike, although the condition appeared at the time to be similar, that is, there was a band, and the one hernia had been reduced around the band, but when it was reduced through its proper canal there were simply two herniæ on the same side, an indirect inguinal and a direct inguinal hernia.

Dr. Collins (Closing the discussion): Dr. Allen B. Kanavel, of Chicago, at the last meeting of the Illinois State Medical Association, read a paper on "Abdominal Crises" in which he said that the general practitioner, who has cases of acute abdominal trouble, does not need necessarily to make an explicit diagnosis; that it will be sufficient to make a diagnosis of whether the trouble is medical or surgical. In acute obstruction of the bowels, it is unnecessary to waste the valuable time to find the explicit cause of the obstruction, but you can decide whether the trouble is medical or surgical and can act accordingly. In the two cases I have reported, an explicit diagnosis proved incorrect, but the general diagnosis that it was a surgical condition was correct, and the operative procedure saved the lives of these two patients.

OPERATIVE TECHNIC IN INTUSSUSCEPTION

SIMON C. BEEDE, M. D.

DAVID CITY, NEBRASKA

The choice of a method of operating for invagination of the bowel can be made by the consideration of factors that are fairly constant. Practically all cases are in a condition of depression that precludes prolonged operation. More than three-fourths of all cases are in patients of tender years who in any condition bear surgical shock but poorly. One quality then of the chosen method must be its brevity, and whatever choice is made this element must be one of the most important considerations. For similar reasons the plan chosen must contemplate the least possible handling of the viscera, for, if visceral manipulation in ordinary conditions adds to the shock and predisposes to post-operative intestinal paresis, how much more will these obtain in the tissues of impaired vitality with which we are dealing?

In common with all forms of mechanical obstruction of the bowels invagination, to be treated successfully, must be operated on early. A recent compiler of statistics estimates that three-fourths of all cases operated on in the first twelve hours recover, while only one-fourth of those operated on at a later period are saved. No dilatory tactics can be tolerated. In this connection I wish to submit that the rectal injection of oil, water, or air, under anesthesia or without an anesthetic, in the horizontal position, the inverted position,

or any other position, is a dilatory measure, as well as an unsafe one, and that its use by the general practitioner is a potent excuse for delay in referring the case to the surgeon and should be discouraged. The universal treatment is by operation, and that operation is abdominal section, if we may exclude those rare cases that may be successfully resected by way of the rectum.

The recurrence of intussusception shortly after its reduction has been sufficiently frequent to make it necessary that the operation performed include such a fixation of the bowel or shortening of its mesentery as will prevent recurrence at this site. Whatever may finally be demonstrated to be the actual cause of death in obstruction of the bowels, experience has shown that drainage of the stagnant fluid in the gut above the point of obstruction promotes the recovery of the patient. Advanced cases of intussusception form no exception to this rule and in them provision for such drainage must be made. Threading the gut on a glass tube introduced into it through an incision into its lumen, is unclean, time-consuming, and involves more visceral manipulation than is admissible in our ideal operation. The method of stitching into the gut a large rubber catheter with rows of purse-string sutures after the manner of a Senn gastrostomy, as repeatedly described by a former president of this Association, is decidedly superior and its principle should be included.

Important as this drainage may be, it is of still greater moment that gangrenous tissue should be removed from the field of absorption. When it is present, resection of the bowel must be done; and as it frequently exists in the irreducible invagination, whether or not it shows upon inspection, it follows that irreducible intussusceptions must always be excised,

and the expedient of anastomosing the gut above with that below the obstruction or of simple enterostomy must never be adopted.

The technic presently to be described is believed by the reader to best meet the indications outlined and is an adaptation of well-known methods, with some variations that have seemed of sufficient value in his own work to warrant their presentation. It does not apply to the occasional case that occurs in adults as a result of intestinal tumors and diverticuli where unusual conditions must be met by measures adapted to that particular case.

The anesthetic must be the one that produces the least depression. Spinal anesthesia has been used in the most remarkable operative cures reported in the literature, but has not found favor with American surgeons, besides having inherent objections to its use in the young children who form such a large proportion of our cases. Gas with oxygen is the ideal, and when its use becomes more general it will be the one chosen. Ether for the present must be our reliance in the average case. Chloroform is especially objectionable in these depressed subjects. The surroundings should be the most favorable for rapid and efficient work, the hospital being chosen if it is accessible. Initial stomach-lavage should be a routine practice. Some cases apparently moribund become fair surgical risks when this is done. Iodine sterilization of the operative field is particularly appropriate here and will not prove unduly irritating if the official tincture is diluted with an equal amount of alcohol, even if the subject is a young baby.

The incision should be of sufficient length to give free access at once to the invaginated bowel. Its location is determined by the tumor, if one is palpable. If no tumor be found, and usually in any event, it

should be made through the right rectus muscle downward from a point opposite the umbilicus. About four-fifths of all bowel invaginations begin in the ileocecal region, to which this incision gives the readiest access. The tumor should be delivered through the incision and isolated with hot moist pads. The intussusception should be reduced by stripping the bowel off the tumor from below. The gut is grasped at the apex of the intussusception with the thumb and finger of one hand and the tumor at its middle with the other, and the hands passed toward each other, thus pushing the penetrating mass backward through the intussuscipiens. The gain made is held by the first hand, and the maneuver repeated till reduction is completed or further progress impossible. Then, and not till then, gentle traction upon the entering gut may be made, if no necrotic tissue has been discovered. It is admissible at this stage to introduce a blunt dissector between the entering and returning layers of the bowel to break up soft adhesions, but cutting the folded edge of the bowel is not justifiable for the reason that, if the edema is here so great as to prevent reduction, the vitality of the part still invaginated will be found so questionable as to demand excision.

The invagination being reduced and the variety being the usual ileocecal whose apex is the cecum and not the ileocecal valve as commonly stated, the meso-appendix is now ligated and severed, and the viscera returned to the abdominal cavity. A blunt forceps is forced into the abdomen through a stab-wound just within the iliac spine, and the appendix caught at its tip and pulled through till the cecum is flush with the abdominal wall and held there by a stitch through it and the skin. If there is much visceral distention the tip of the appendix is snipped off and a soft rubber catheter passed through its lumen into the small intestine, the finger guiding it through the ileocecal

valve. The abdominal incision is closed with retaining sutures of silkworm gut and layer sutures of catgut. The appendicostomy affords a ready means of draining the bowel and introducing fluid for absorption. Should the distention prevent a ready return of the viscera into the abdominal cavity it can be rendered easier by introducing the catheter first and partially emptying the coils through it. Should drainage of the bowel not seem necessary at the time it can still be utilized in the hours succeeding the operation should it become desirable. In any event it fixes the cecum firmly so that that variety of intussusception cannot occur again and disposes of the appendix, which at least sometimes excites it. In a few days it can be snipped off flush with the skin, and it soon disappears in the scar.

Should the invagination prove to be irreducible it should be excised from within the enveloping gut; but first as much reduction as possible is made. If a gangrenous area is uncovered it should be re-invaginated. A seromuscular suture unites the bowel below to that above at the point of entrance of the intussusceptum. This is of linen and begins opposite the mesentery. When that structure is reached the needle is passed through the mesentery of the entering layer at its junction with the bowel and back again through all three layers, then forward again at the point of first penetration, and on to the place of beginning, where it is tied. A longitudinal incision is now made opposite the mesentery an inch or more below the line of suture, and the intussusceptum delivered through it, the intestine above having been clamped after its contents have been stripped back to prevent leakage. A protected clamp is now placed on the base of the tumor at the suture-line, and the mass cut away about a quarter of an inch from the clamp. If the intussusceptum is long it may be clamped and cut away

before delivery. The clamp now embraces the entering and returning layers of the gut with their mesenteric attachments lying between them. A through-and-through continuous suture is made to unite them and prevent hemorrhage from the cut mesentery, and the clamp is removed. A large soft rubber catheter is passed into the gut above the suture line through the longitudinal incision, which is closed with Lembert sutures, beginning at the lower end. When the catheter is reached at the upper end of the seam, it is continued around it two or three times as a purse-string suture, invaginating the bowel-wall about the tube as it is tightened and tied. The abdominal wound is closed with retaining sutures of silkworm gut and layer sutures of catgut. The first peritoneal stitch catches the bowel immediately above the catheter and maintains it in apposition with the abdominal wall. One wormgut suture is tied about the catheter to prevent it from slipping out. In cases not demanding bowel drainage this feature may be omitted.

If the condition is desperate and the few moments required to make the initial peritoneal suture an added peril, it may also be omitted; but the additional safety it affords justifies its routine use. However, the second through-and-through suture alone is very secure, as against both leakage and hemorrhage. It should never be displaced by the glover's stitch in this situation. This method is applicable to the majority of cases requiring resection, and meets the indications outlined. Recurrence in the same place is impossible when resection is made. It is secure against leakage and hemorrhage. It can be done more rapidly than any other resection. It saves all the healthy bowel possible and preserves some that by any other method must be sacrificed. The only tissue exposed to soiling is below the obstruction and hence the most resistant. It exposes the least possible amount of viscera to trau-

matism. It provides for drainage of the distended bowel. An extensive study of the literature shows that its essential features have given the best results.

But some cases will require resection by some other method, as, for instance, one in which the vitality of the intestine has proved to be deficient after reduction has been accomplished. The choice then is a closure of the divided ends of the entrail and a lateral, anastomosis of the two limbs. This can be done more expeditiously than an end-to-end union in these cases, as the tubes to be united are of different caliber, either because the anastomosis is between ileum and colon or between the thinned-out part above the obstruction and the contracted part below. It is more secure as it avoids the danger of a leak in the suture-line at the mesenteric border. The technic is so familiar that it is rehearsed only to emphasize some time-saving features.

The portion to be excised having been determined, two clamps are placed on each end of it, about one-fourth inch apart. One pair of clamps is now taken in the left hand while a peritoneal stitch of linen encircles the healthy bowel about three-quarters of an inch from the line of section, beginning and ending at a point opposite the mesentery. The bowel-contents are now stripped back a few inches and held away from the operative field by a guarded clamp on the intestine at this point. The gut is now severed with a knife, shaving close to the clamp on the part to be removed and the ends dried with gauze. A catgut ligature is thrown about the end to be closed beneath the clamp and tightened and tied as the latter is loosened and removed, the ligature thus falling into the groove made by it. The ligated stump is inverted into the bowel beyond the encircling suture, which is tightened as a purse-string and tied, the ends being

left long to serve later as tractors. The other line of section is treated in the same manner. By making traction upon the long ends of the purse-string sutures and holding firmly the two clamps that were placed upon the bowel to prevent leakage, the two limbi which are to inosculate lie side by side like two ribbons with the antimesenteric borders in the most favorable position for the anastomosis, which is now made in the usual way without the use of further mechanical aids. The exsected part of the bowel still has its ends closed by the forceps, and it is not removed, but is used to put the mesentery upon the stretch. This structure is now under easy control and can be clamped and tied off with great facility, and its edges sutured together. Should a bowel-drain be necessary the clamp is removed from the upper limb and a catheter inserted before the ligature is tied, and an additional purse-string suture applied. The drain-tube itself then furnishes the most convenient instrument for inverting the stump. This limb is then sutured to the peritoneum, and the wound closed in the manner already described.

DISCUSSION

Dr. Daniel N. Eisendrath (Chicago): I think our mortality in cases of intussusception is already so high that it seems foolish to increase it by any unwise measure, which we should do if we followed exactly the advice of Dr. Beede, and it is for that reason that I take this opportunity to say something on this subject. Anyone who has operated on an infant or child with intussusception and has seen the degree of shock which these children have, knows that the shock is so great that the least amount of operative interference is the thing that will give the greatest amount of mortality. The statement that every child of this kind should have the intussuscepted portion of the bowel resected, I do not think ought to be allowed to go forth from this Association, because we are sure to have a mortality of 100 per cent, or practically so. In those cases where you cannot pull the intestines apart, and do that in the first twelve to twenty-four hours, and feel safe in doing so,

it is much better, if the child is in any degree of shock, to bring the intussuscepted portion of the bowel out through an abdominal incision, do an enterostomy, tide the child over for two or three weeks, and then do a resection at leisure when the child is in good condition. This is a much safer procedure.

As to the method of performing enterostomy: The Kader method, which is the use of a double purse-string, is not as good as the Witzel. This method is one by which we take a catheter, lay it in between the layers of the bowel, and then put it in at the last moment, fixing it in the end of the bowel with a purse-string. I have seen many children die where I have tried to do too much. These children will not stand resection, and the less we do the better it will be.

Dr. Clifford U. Collins (Peoria, Ill.): There was one part of Dr. Beede's paper which impressed me, and that was that these cases should be operated on inside of twelve hours if we expect to decrease the mortality very much. I believe that is true. Surgical technic has been improved up to the point where the mortality will not be lessened very much unless there is still greater improvement in the surgical technic. We must get the cases for operation sooner.

In an article which I shall read before the Illinois State Medical Association in May, I make the point that the general practitioner must be educated so as to bring these cases to the surgeons sooner than he has been doing. It will not be sufficient for him to see the case, and if there is obstruction to give morphine and not see the patient for twenty-four hours. He should remain in touch with the case, and should make the diagnosis as to whether the trouble is medical or surgical, and if it is a surgical case, the patient should be brought to the surgeon as soon as possible.

The essayist said that when gas and oxygen anesthesia came into more general use, it would be an ideal anesthetic for the class of patients of which he was speaking. I do not think gas and oxygen produce sufficient relaxation of the abdominal muscles to be an ideal anesthetic for this condition. Dr. Teter once said it should not be given to a child less than two years of age, and a great many of these cases will occur in children younger than that. Those who use gas and oxygen in abdominal cases likewise use ether with it to produce relaxation. This may not sound well coming from a man who advocated gas and oxygen anesthesia a year ago, but with increased experience I take occasion to say that it will not be the anesthetic of choice for these patients.

Dr. William D. Haggard (Nashville, Tenn.): I did not expect to be called upon to discuss this paper. I have enjoyed it very much, the part of it that impressed me most being the insistence on the part of the essayist that the practitioner should avail himself of every opportunity to complete a diagnosis of intussusception. That is where the whole danger lies. A diagnosis of intussusception should be made by abdominal section early. It is such a clean-cut thing in the majority of cases that this will not be necessary.

Recently a statement was made of a medical man who has devised an ingenious method of reducing intussusception in infants without abdominal section by a certain laying on of hands under anesthesia. It is well known to all of you that in many instances it is impossible to reduce the intussusception when you have the intestine in hand; therefore it seems to me far from rational to promulgate such a practice as the attempt to reduce an intussusception in a child in that manner.

In reference to the resection of the intestine: The remarks of the essayist apply only to those cases in which the intussusception could not be reduced, and I think all the members of the Association here are aware of the fact that no case of resection has been successful in the infant. Therefore that suggestion in these cases would be unwise. I think, too, that the essayist's contention that the enterostomy is extremely valuable in these cases, as an adjunct is sound practice, and, moreover, it is preferable to the use of Moynihan's long tube. Those who have used that tube know that while it is theoretically excellent it is a difficult procedure to thread that through a number of feet of distended intestine, for the reason that when you touch it the peritoneum breaks. The use of the appendix is to be particularly commended as a drainage route for enterostomy.

Dr Beede (closing the discussion): I wish to thank the gentlemen for the discussion they have given this subject. When the matter first came to my attention for particular study I did not know of any case of irreducible invagination of the bowel in a child under one year of age that had lived. It was said by a member of this Association, in a discussion of this subject a year or two ago, that there had been no case reported of irreducible intussusception in a child under one year of age that had recovered. Since then I have seen reports of two cases in which resection of the bowel had been successfully accomplished in children, one of six months and the other a month or two older or younger. They were reported in the London *Lancet* about two years ago, and the method used, I

am glad to say, was the one I have recommended here, namely, excision of the intussusception from within the bowel.

I am glad to know that Dr. Eisendrath has been able to save some of these young children by enterostomy, and then later resorting to resection of the bowel. Study of the case-reports does not encourage this procedure, and my own experience certainly does not. I have not been able to find the records of any such recovered cases in the literature at my command, though operative cures in older children are common enough. If there are such cases reported, I should like to know it.

The mortality of intussusception in infants is frightful, and I believe it is possible by studying the technic of the operation to improve our mortality-rate in these young patients.

OBERVATIONS ON GERMAN SURGERY

Charles H. Mayo, M. D.

ROCHESTER, MINNESOTA

During the past summer it was my privilege to make a most instructive and enjoyable visit to some of the renowned clinics of Germany. Twenty-five university centers in Germany seem to be classified according to equipment, wealth, number of students, and extent of population. As opportunity arises the chiefs of clinics are advanced in the scale of appointments, or they pass on to other universities and higher positions. The better of the first assistants are chosen to fill the vacancies.

The hospitals are large and complete as to equipment, usually occupying a space of about four of our city blocks. The large park-like grounds with plenty of air and sunlight make ideal surroundings for the hospital buildings, which are usually constructed on the pavilion system. The location of hospitals in Germany is in marked contrast to that in our own country, where hospitals are often built in the business centers without ground space, as modern skyscrapers, with the discomforts of heat, smoke, dust, and noise, air and light being secondary considerations.

The Virchow Hospital in Berlin has been constructed recently and is a model in detail. However, in Frankfurt-am-Main is one of the best equipped and most modern hospitals that I visited. It has been remodeled from an old institution. Rehn is the chief surgeon here, and he has a large clinic.

The hospitals are all well equipped with modern Roentgen apparatus and this department of diagnostic work seems to have been developed to a wonderful degree of perfection. In Frankfurt we saw excellent radiograms taken in $\frac{1}{200}$ of a second. The work of Haudick of Vienna in the reading of the radiograms and in the interpretation of fluoroscopic pictures in cases of gastro-intestinal diseases, has occasioned much interest.

A great deal of care and thought has been given to the arrangement of the operating-rooms, and especially to the lighting system, several of them having the so-called Jena system, with light from a powerful arc focused onto mirrors. In some instances these mirrors are fixed, and in others they can be tilted. There is considerable water in evidence in most of the operating-rooms, but on the whole they are much drier than during the antiseptic period of surgery. In most of the clinics the immediate site of operation is prepared by the benzine and iodine method.

The surgeons are exceedingly well prepared for their work. They have served long apprenticeships, and they all take pride in speaking of the well-known men with whom they studied and served as assistants. They are especially well informed concerning the world's medical literature. As a rule, they may be said to take themselves and their work rather seriously and impress the fact upon others. Very few of the surgeons change their clothing before operating, often taking off but the coat and vest, slipping a rubber gown over their clothing, and drawing a pair of short rubber boots over their footwear.

In all the operating-rooms are a great many assistants, and after the essential details have been carried out by the master surgeon, the case is passed on to the first, second, and third assistant surgeon, who com-

pletes the work. In Bier's clinic in Berlin which is one of the largest in Europe, we counted twenty-seven persons,—surgeons, internes, orderlies and nurses,—engaged in operating upon or completing the work on three patients at one time. There was opportunity to visit but three of the general surgical clinics in Berlin which were conducted by Bier, Hildebrand, and Körte. We also visited the clinics of Bumm and Franz, which are devoted to gynecology, and that of Krause, who specializes in the diseases of the brain and nervous system. He is also doing much general surgery.

The excellent team-work of those engaged in diagnosis in all of the departments is worthy of note, each diagnostician taking great pride in the part he plays in the making of a diagnosis. He follows his cases to the operating-table to witness the conditions found upon exploration. In abdominal surgery much time is given to making an exact diagnosis, rather than the diagnosis of a surgical condition, and the operation is carried out strictly with reference to this.

Very little general exploration is made, although the incisions for abdominal work are generous ones, permitting ease of investigation and manipulation. Such incisions do not appear necessarily to lead to a long confinement in bed. The period of convalescence has been greatly shortened, particularly in Kümmell's clinic in Hamburg, who has most of his patients, not previously bed-ridden, up for a few minutes the day after operation and for an increasing period each day thereafter.

Franz of Berlin, an exceedingly clever operator, makes use of the transverse or Pfannenstiel incision in abdominal cases, and secures a good exposure after a vigorous stretching of the incision. He favors spinal anesthesia.

Many cases of diseases of the stomach are to be

seen in all of the clinics and a correct diagnosis of the condition is the rule.

Ulcers are often treated by resection of a complete ring of the stomach, including the ulcer. The two portions of the stomach are reunited and a posterior gastro-enterostomy is made when there is interference with the drainage of the stomach.

In carcinoma of the stomach partial gastrectomy by the Billroth No. 2 method is a common operation, although Hofmeister of Stuttgart uses a modified Billroth No. 1 by attaching the jejunum. He protects the fatal point of union of stomach and jejunum by continuing the suturing of the small intestine over the suturing of the stomach, the jejunum being brought through the mesentery of the transverse colon to which the anastomosis is attached by sutures at the close of the operation.

They have evidently had but little trouble in German clinics from hernia of the small intestine through the opening in the transverse mesentery of the colon following gastro-enterostomy for ulcer since rarely did we see sutures applied to prevent the possibility of this accident.

Free use is made of the Pagenstecker linen, silk, or catgut suture material, some of the surgeons making an anastomosis in a very few minutes with a single row of interrupted mattress sutures of catgut or silk; others, however, use the double row of sutures with the third row posteriorly for a more careful control of hemorrhage.

Bumm in Berlin and Wertheim in Vienna make complete abdominal operations for cancer of the uterus. In Schauta's clinic a perineal operation was apparently just as radical, a very large exposure being made by a lateral vaginal incision. It is generally admitted that,

when the disease has advanced outside of the uterus with glandular involvement, the percentage of five-year cures is very small indeed, and that delay in the continuance of growth (I do not call it recurrence) depends largely upon the resistance of the individual. The somewhat increased mortality necessarily following tests the best judgment of the operator in deciding how radical a procedure is warranted in these advanced cases.

Many operations on the gall-bladder and ducts were seen in the various clinics. Most of these cases are operated upon somewhat later, on the average, than in this country, which undoubtedly has led to the higher percentage of cholecystectomies there than we have here. Perthes and Pyer use a modified Kocher incision, which gives a fine exposure of the ducts. The incision extends from the ensiform cartilage to the umbilicus, and from the umbilicus at a right angle as far as the costal margin if needed. The rectus muscle is divided after being sutured to its sheath in two rows lateral to the proposed incision to prevent retraction.

An interesting point in the causation of gall-stones has been worked out in Payr's clinic in Leipzig. He shows that the percentage of cholestrin in the bile of pregnant women is four times as great as in other women or in men, and, as 19 per cent of the weight of the gall-stones consists of cholestrin, he believes that this accounts for the fact that such a large percentage of the cases of gall-stones occur in women, and that the first symptoms have become manifest in the majority of them during pregnancy or shortly after.

In three instances Wilms has made an anastomosis of the hepatic duct and duodenum with a permanent rubber tube. The first case was done three years ago. He presented two of the cases at the clinic.

German surgeons have a great liking for recon-

structive work, and much can be learned in a study of their bone, joint, and plastic surgery. Lexer of Jena showed a number of cases in which he had transplanted the entire knee-joint immediately removed from amputated limbs to replace anklosed ones. A very useful degree of motion was present in the several cases shown. Payr is making resections of fixed joints to secure motion after Murphy's original plan, removing more from the femur than the head of the tibia, however, which is the reverse of the present method. He transplants the fascia lata and its fat into the joint in the same manner. In the end-results of the operation, the radiogram shows but little difference between the methods, both permitting of a near function from like changes in the different tissues.

A very common plastic operation is that of transplanting fat to fill out defects of the skull and the face, as well as the free use of transplanting the fascia lata with the fat side down in operations on the brain, especially in epilepsy, the dura over the exposed area being entirely cut away. Perthes transplants hernial sacs, removed at operation for this purpose, into ankylosed joints. They claim greatly improved results by such methods and have much less trouble from adhesions and cystoid scars than followed former operations upon the brain. Hildebrand decompresses by aspiration of a ventrical through the corpus callosum. The skull is opened with a small drill.

In three clinics we observed an artificial esophagus made of a section of small intestine. It was connected with the stomach, extending over the sternum beneath the skin and continuing on to the neck as a tube lined with skin, which was connected with the esophagus, three operations being required. Lexer's case was the only completed one, and it was interesting to see the bolus of food go sliding down through this artificial esophagus over the front of the chest.

In all of the cases the stricture was non-malignant. Enderlen, in a similar case, united the esophagus with the stomach, which was drawn through the diaphragm. The patient died on the eighth day of pneumonia; the autopsy did not show any infection; healing of union was perfect.

He also reconstructed the alæ of the nose with the lower lobe of the ear, and exhibited many bone and joint reconstructions.

Perthes of Tubingen and Wilms of Heidelberg showed many cases of surgery of the lung, especially partial removal of the rib in tuberculous conditions to replace the lung at rest. The advantage claimed over the Friedreich operation by the removal of short sections of several ribs, front and back, through several incisions, was the slower shrinking of the wall of the chest. They use differential pressure in interior operations on the chest.

Bonn is a most interesting university center. Professor Garre, one of the best surgical teachers of Europe and an operator of excellent judgment, is in this university. Professor Makkas presented a patient in the clinic with exstrophy of the bladder in whom, three years before, he had devised a bladder from a cecum, which was separated from the colon after making an ileocolostomy. The ureters were connected with it, and the bladder thus formed was drained every three or four hours by a catheter passed through the lumen of the appendix, which had been converted into a urethra by an appendicostomy. However, in the radiogram just taken, the ureters showed dilation extending to the kidney.

Goiter is a very common disease in Germany and operation of frequent occurrence. The methods of operation are similar in plan in all of the clinics—transverse incision, double ligation, and division of all of

the tissues over the gland, and very careful ligation of the arteries with some strong suture material. Few surgeons trust catgut for this purpose, and use silk, linen, or silkworm gut. In Germany exophthalmic goiter is considered a surgical disease, and it was interesting to see so many cases in the various hospitals. Preliminary ligations of the arteries are made in most of the clinics when the disease is operated on in an active stage. Rehn now removes one lobe of the thymus gland, if found, with the extirpation of the thyroid. Most excellent results are obtained by operation, and the mortality is reduced to a very low percentage. The surgical treatment and the results are borne out by the work of their physiologists also.

A general knowledge of the development, function, and therapy of the various ductless glands of the body seems to have been worked out here with much less bias or theory than in other countries.

In Leipzig a most interesting visit was enjoyed with Professor Trendelenburg, a well-remembered visitor to America, now retired from active practice. Here also Professor Tillmanns showed some very interesting cases at the Children's Hospital. Many varying tuberculous lesions were being treated with tuberculin to which was added the staphylococcus toxins. The patients showed marked improvement, claimed to be due to a more active local inflammatory reaction.

In Frankfurt-am-Main an interesting visit was made through the laboratories of Paul Ehrlich, one of the foremost men in the profession today. He is a small, active, jolly, fellow, most interesting to meet and a typical scientist.

In Vienna the average attendance of American surgeons at the clinics is about 150. They maintain a continuous local society, deriving inspiration from the excellent surgery of von Eiselsberg (a student of Bill-

roth and a clear-cut surgeon), Hochenegg, Clairmont, Zuckerkandl, and other world-famed surgeons and clinicians of that city. The atmosphere of the clinics is quite American.

The details of anesthesia varied in the different clinics. As a rule, the operating-rooms are supplied with an apparatus which delivers to the patient oxygen, ether, and chloroform in exact proportions. Körte in Berlin was using the old A. C. E. or Billroth mixture. He claimed that ether alone presented some difficulties when used on those patients who were accustomed to their "schnapps." While noting the excellence of general anesthesia as produced by the various methods (ether as a drop-method is used much in Vienna), we were impressed with the wonderful development of the various methods of local anesthesia that have been perfected in the several clinics. One could not but feel that there had been some necessity for it owing to trouble in the past with general anesthesia. The best local anesthesia resulted when twenty to thirty minutes were allowed to elapse between the injection and the operation. Abdominal operations of all kinds were made under intradural spinal anesthesia. The skull was opened and an operation on the brain was made under local anesthesia. The middle and posterior branches of the fifth nerve were injected at the base of the skull, and in some instances the ganglion has been reached with a curved needle, permitting of most extensive resections and other operations on the face and jaws. Operations on the limbs, bones, and joints were made by brachial plexus and sciatic nerve injections of novocain, the nerves being found by an insulated electric connected hypodermic needle. They use the pneumatic tourniquet of Perthes distended within a metal shield, which gives perfect control of circulation, and is somewhat similar to the apparatus used by Bunts of Cleveland.

Kümmell made a partial gastrectomy and gastro-enterostomy using five per cent ether in salt solution, given intravenously in the arm. A continuous stream of ether was introduced when needed, and salt solution when anesthesia was sufficiently deep, 16 to 18 grams being used during the operation. Evaporation probably reduces the strength of this to about 3 per cent when received by the patient.

Prostatectomies and operations for hemorrhoids were made by an extradural spinal injection, each side of the sacrum and coccyx. Wilms made very rapid perineal prostatectomies, one in three minutes and another in four minutes, using a long needle and deep injections of novocain about the base of the bladder and prostate, the needle being inserted through the perineum with the finger in the rectum as a guide. A number of herniotomies were made by the Heinrich-Brown method of local anesthesia, four deep penetrations of the needle and injections of novocain beneath the aponeurosis, each two inches lateral to the ends of proposed incision, with local skin injection along the line of incision.

A case of osteomyelitis of the tibia in a fourteen-year-old boy was operated on after securing anesthesia by opening the saphenous vein in the thigh below a tourniquet and injecting it, under tension, by a syringe filled with novocain solution.

All of these methods showed to what extent an operation can be carried out successfully without the use of general anesthesia when it is contra-indicated.

German surgery can well be spoken of as a "school of surgery," and has more or less dominated the surgery of the world. The German surgeons are most efficient. While their surgery has originally developed almost entirely from the laboratory side, the clinical work also has certainly been advanced remarkably.

The methods of hospital development, support, and control have created a system which is adapted to the masses in contrast to that in this country, which considers the individual. Although many of their methods are not adapted to our institutions and people, they are well worth consideration by our surgeons, as there is much to be learned from them.

AN EXPERIMENTAL STUDY OF SODIUM BICARBONATE AND OTHER ALLIED SALTS IN SHOCK

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In the recent literature there is a vast preponderance of work on the various possible causative agencies of shock and practically a dearth of investigations directed toward the therapeutic relief of this serious symptom complex. Howell[1], in 1903, was led by a laboratory accident to investigate the physiological action of sodium carbonate and found this salt to be an excellent cardiac stimulant in shock. Henderson[2], in 1910, as a result of his extensive investigation of acapnia, suggested as a rational therapy for shock, the prevention of loss of CO_2 (the CO_2 being lost chiefly as a result of hyperphia and by exhalation from exposed viscera), and the replacement of such loss, as had previously occurred, by increasing the dead space of the respiratory tract. These two investigators furnish about the only new lines of therapeutic thought developed during the past decade, on the subject of shock.

Howell's work is important in that it demonstrated both the pronounced action of sodium carbonate on the force of the heart-beat and also the duration of this effect over a comparatively long period of time. According to Howell, the rate of beat was unaffected, and

the vascular tone, likewise, in all probability, was not influenced. In severe degrees of shock, sodium carbonate did not yield the brilliant results secured by the use of this same drug in the more moderate cases (where the blood-pressure registered 60-70 mm. of mercury), but even so, to quote Howell, the results were more hopeful "than those obtained by the use of other substances suggested as having value. Adrenalin, hypophysis extract, alcohol, and strychnine gave negative results or, at best, temporary and unimportant rises." The nucleus of Howell's work, though not expressly so stated by the author, lies in the thought that the pressor effect of sodium carbonate is due to its alkalinity. Dawson[3] extended Howell's work by using sodium bicarbonate, but in his experiments he induced shock by bleeding his animals, thus complicating an already complex set of phenomena by the added element of hemorrhage. Henderson's work, in its turn, is important in that it reviews and reëmphasizes the importance of carbon dioxide as a body harmone, and particularly in that it points out the absolutely fundamental fact that, in all probability, the low blood-pressure which results from venopressor disequilibrium, is not the principal causative factor of shock, as it is so generally assumed, but is rather the direct result of shock. Henderson's main line of thought may be sketched in as follows: As a result of various factors,—pain, excessive flow of afferent impulses, or exposure of viscera,—there is a loss of carbon dioxide, either by excessive lung-ventilation or by direct exhalation from the viscera. Carbon dioxide is an important, though poorly understood, regulator (harmone) of many of the vital functions (cardiac, peristaltic, respiratory, venopressor), consequently when the loss of CO_2 becomes quantitatively sufficient, these vital functions are thrown out of gear up to the point of compromise or further, even to cessation of ac-

tivity. Contrariwise, balance may be restored by methods adapted to increasing the deficient CO_2 content of the blood. Henderson's views have been vigorously attacked by physiologists and his own data have been used to disprove his main thesis. But leaving aside all polemical discussion, it may be stated fairly that if, *in accordance with Henderson, shock is caused by acapnia or a lessened carbon dioxide content in the blood, then the direct introduction of carbon dioxide into the blood ought to be a powerful and fairly certain remedial agent.**

On the above assumption we based the following series of experiments. Before describing our experiments, however, it must be stated that two essential facts were kept constantly in mind, viz.: (1) Blood-pressure readings in shocked animals must be interpreted merely as expressing phenomena *coincidental with,* and not necessarily *causative of,* shock. Attention is centered largely on blood-pressure because it is conveniently measurable accurately and graphically; and because, in a sense, it furnishes an index of the degree of shock. In the anesthetized dog, there are remarkable spontaneous variations of pressure, which, as far as can be determined, have nothing except a coincidental relationship with procedures aimed at either producing or relieving shock. These variations in pressure will necessarily be falsely interpreted unless a large number of control experiments are performed, as routine work†.

*Henderson himself attempted to increase the CO_2 content of the blood by two methods, both of which are open to criticism. His method of infusing physiological salt solution saturated with CO_2 is open to question on account of the bulk of fluid necessarily injected (Henderson does not furnish figures or details of these infusions), and his method of increasing the dead space of the respiratory tract is open to criticism because it is not accompanied by analysis of the lung and blood gases before and after.

†The conclusions on which this paper is based rest upon a hundred-odd experiments performed on forty-six dogs. All the experiments were performed with the animal under profound morphine-Grehant (chloroform) or morphine-either anesthesia. We are deeply indebted to Dr. B. F. May for valuable assistance rendered during the summer of 1911 in a large series of experiments.

Assuming the correctness of Henderson's assumption, that shock is due to a deficiency of carbon dioxide in the circulating blood, we attempted to check up his theory by supplying the blood-stream directly with an increased quantity of this gas. For obvious reasons, the demands of accurate experimentation would not be satisfied by infusing a solution of CO_2 in water or in an isotonic medium. In our earlier experiments we were deterred from introducing pure CO_2 gas into the circulation by the fear of gas emboli, therefore it was determined to use a molecular solution of sodium bicarbonate. There are three elements in the blood which split up the sodium bicarbonate molecule and set free CO_2: (1) the hemoglobin; (2) the serum albumin; and (3) the primary (acid) sodium phosphate. It will be seen therefore that the introduction of a solution of bicarbonate into the blood-stream furnishes a direct and immediate increase of the carbon dioxide content.

We may say at the outset that, without exception, every infusion of sodium bicarbonate caused a pronounced rise of blood-pressure. In our first experiment, in which we introduced intravenously 200 cc. of a 8.4 per cent solution, the rise was so remarkably high, so long-continued, and so notably in contrast with the efficiency of all the other antishock drugs and procedures that we had tried during the past five years, that the practical therapeutical phase of the problem almost overshadowed the purely scientific inquiry regarding the influence of the carbon dioxide content of the blood. A molecular solution of sodium bicarbonate was injected, in all, fifty times, under varying conditions of blood-pressure. An analysis of the results obtained from this set of experiments furnishes the following facts: (1) When 25 cc. of the solution is injected into a normal (anesthetized) dog, with normal blood-pressure, it occasions no rise of pressure

and no perceptible increase of amplitude of beat. When, however, the injection is made into a dog reduced to a state of shock by manipulation and chilling of the abdominal viscera, there is an immediate rise of pressure. Quantitatively, this rise depends very largely upon the age and general condition of the dog (old dogs respond much less vigorously than do young ones), and also upon the degree of shock. In moderate shock, the rise is much more pronounced than in the later stages. We consider a rise of 18 mm. (mercury manometer) to be an average effect, but it was not uncommon for an injection to be followed by a rise of 40 mm. It was noted in analyzing our tracings that more significant than the sharp response to the injection is the steadiness with which the blood-pressure climbs and maintains itself at a high level. We found, in many instances, that after the new level of pressure was established, it required rough manipulation to reinduce the previous low level of shock-pressure; and we believe that those dogs that received an initial injection of sodium bicarbonate before the abdomen was opened and while the pressure was at its normal height, were with more difficulty reduced to a state of shock than those animals that did not receive an initial dose. Such a statement, of course, is difficult of proof because of the varying resistance of different animals to the various manipulations we practised.

Hand in hand with the rise of pressure went an increase of amplitude of heart-beat. This increase varied quantitatively, as did the rise of pressure itself, its chief characteristic being its constancy. Not infrequently the amplitude of beat was doubled immediately after the injection of the bicarbonate, and the increase of amplitude often persisted for half an hour or longer after the injection of the solution. We were fortunate in encountering one dog (Experiment 16) that showed an abnormally large amplitude of beat be-

fore shock was induced. In this animal the amplitude of beat was 24 mm. In the state of shock the amplitude fell to 15 mm., but, following the injection of 25 cc. of a molecular solution of sodium bicarbonate, the amplitude rose to 21 mm. Likewise, in some animals very susceptible to manipulative procedures, with a normal low amplitude, in whom the cardiac weakness was so pronounced as to barely record any systolic-diastolic differences on the tracing, the injection of the bicarbonate solution was immediately followed by an amplitude of beat measuring three, four, and five millimeters. This increase of amplitude, as we have pointed out in the case of the blood-pressure rise, is also well maintained over a fairly long period of time. One experiment (Experiment 19) was particularly instructive in demonstrating the contrasting effects of sodium bicarbonate and adrenalin, one of the stock antishock drugs. The immediate effect of the first adrenalin ($\frac{1}{5000}$) injection was to raise the blood-pressure without influencing the low amplitude of beat. Following the injection without an appreciable time interval, the blood-pressure fell *to a lower level* than before the injection, and at this level it remained. A second injection of a stronger solution of adrenalin ($\frac{1}{1000}$) caused a more marked rise of pressure and a pronounced increase of amplitude lasting one minute, followed by a fall of pressure again to a lower level than before the injection and a diminution of amplitude to its previous small height of 1 mm. At this stage 50 cc. of a molecular solution of sodium bicarbonate were injected and were followed by a *well-sustained* rise of pressure and by an increase in amplitude up to 4 mm.

In all of these fifty experiments with sodium bicarbonate we could not determine any effect on the rate of heart-beat. When the bicarbonate was administered before shock was induced, the rate of beat was not

quickened, and when it was administered during the period of rapid heart-action, incidental to shock, the rate was not slowed. These facts tally with the results of Howell (*loc. cit.*) in his study of sodium carbonate.

In addition to its influence on the cardiovascular apparatus, the sodium bicarbonate infusions exerted a marked effect on the respiratory function. The rate of respiration was sometimes increased, sometimes slowed, and sometimes unaffected. These variations did not seem to depend upon idiosyncracies in the different dogs, for we observed all these variations in the same dog following different injections of the same amount of the drug. The notable fact was that the depth of respiration was increased. This respiratory response which followed almost immediately upon the injection of the salt and persisted for a long period after the injection had been made, was so pronounced as to attract attention by the marked thoracic movements and the deep sighing respirations. Graphic tracings were necessary merely as a matter of record. The explanation of the increased respiratory activity undoubtedly rests upon the influence of the excess of CO_2 upon the respiratory center, for the respiratory phenomena observed after the injection of sodium bicarbonate were practically identical with those noted after the injection of pure CO_2, in later experiments. By using a marked excess of sodium bicarbonate solution the respirations could be made feeble and shallow and, for short periods, even inhibited.

Such, then, were the effects noted upon the cardiovascular and respiratory mechanisms, following the injection of small quantities (25 cc.) of a molecular sodium bicarbonate solution; marked increase of blood-pressure, the increase following immediately upon the start of the injection and being well sustained for varying long periods; no influence upon rate of heart-beat; marked increase of amplitude of beat, this increase

likewise being well sustained over varying long periods of time; varying effects upon rate of respiration; constant effect upon depth of respiration, which was so increased as to be noticeable without the aid of tambour and drum.

It seemed to be evident after the first experiment that the pressor effect of sodium bicarbonate could hardly be explained on our *a priori* assumption, that CO_2 was liberated intravascularly, thus replacing the CO_2 lost as a result of shock (acapnia, Henderson). For, as we have already noted, coincidently with the rise of pressure following the injection of the bicarbonate, there set in a period of marked and sometimes violent respiratory activity that induced a more thorough and prolonged ventilation of the lungs than we have ever seen caused by procedures planned to induce shock. If Henderson's theory of acapnia were correct, such ventilation ought to have deepened the shock of our animals (25 cc. of our solution contained 2.1 g. CO_2, which could hardly furnish enough CO_2 to make up any marked deficiency of CO_2 in the blood) on account of the marked increase in respiratory activity and resultant pulmonary ventilation. Since, therefore, the rise of blood-pressure could not, with certainty, be explained on the basis of increased CO_2 content of the blood, we tried to determine what other factors might be discovered as possible explanations of the pressor effect of sodium bicarbonate. We attacked the problem by the process of exclusion and took up, in order, the influence of (1) the bulk of fluid injected; (2) hypertonicity of the fluid injected; (3) alkalinity of the fluid injected; (4) influence of free CO_2 (intravascular) on the blood-pressure.

Bulk of Fluid Injected.—It is unquestionably true that the intravascular addition of any appreciable quantity of fluid to a shocked animal will cause a rise of pressure. This rise is usually transitory, and the tran-

sitoriness usually agrees directly with the degree of shock. In the marked grades of shock the fluid escapes into the tissues (edema) about as rapidly as it is introduced into the vessel, and therefore causes but slight or no rise of pressure. In order to avoid any confusion of interpretation, such as might arise from mere bulk of fluid injected, we always introduced amounts under 26 cc. We are therefore in a position to state that the rise of pressure following our injections was not due to mere bulk of fluid. In a number of experiments we noted a distinct rise after the introduction of from 2 to 5 cc. of a normal solution of sodium bicarbonate. In this connection, one series of experiments furnished particularly interesting results: A given quantity of sodium bicarbonate (2.5 grams) was injected into the shocked animals in dilutions of 1 to 10 and 1 to 40. The animals that received a 1 to 10 dilution (i. e. 25 cc.) showed the typical well sustained rise already described, whereas those that received a 1 to 40 dilution (100 cc.) showed a much more pronounced initial rise following the injection, with a tendency to sustain the high level. The greater rise caused by the more dilute solutions can be attributed only to the excess of fluid (75 cc.) introduced, for in both instances the quantity of sodium bicarbonate was the same.

Influence of Hypertonicity.—It has been demonstrated by numerous observers that in shock there is an escape of plasma from the blood-vessels into the tissues. This observation has been confirmed at autopsies and also by the determination of the specific gravity of the blood, which is heightened in shock as a result of concentration of the blood. It has even been suggested that shock is due to increased specific gravity of the blood. If the current of the stream of liquid from the vessels toward the tissues could be reversed, or even stopped, the specific gravity of the blood would not be altered. It is possible that the introduction of

a hypertonic solution might accomplish this purpose, and that our sodium bicarbonate infusions might have been effective on this basis. That this assumption is not correct, however, we proved by the simple experiment of infusing shocked dogs with hypertonic salt solution and with hypertonic basic sodium phosphate solutions, with practically no effect on the low blood-pressure. Dawson (*loc. cit.*) working with hypertonic sodium bicarbonate solution secured results identical with ours.

Influence of Alkalinity.—It is so natural to assume that any phenomenon following the use of an alkaline salt is due to alkalinity that we must guard against a *post hoc* error in logic, in explaining the pressor effect of sodium bicarbonate on the basis of its being an alkaline salt. In order to determine the influence of alkaline salts on the low blood-pressure of shock, we infused various dogs with small quantities (25 to 50 cc.) of $\frac{1}{10}$ per cent, $\frac{1}{4}$ per cent and 1 per cent of potassium hydrate in physiological salt solution with half molecular solutions of basic sodic phosphate ($NaH PO_4$), with half molecular solutions of sodium carbonate, and with molecular solutions of the alkaline tribasic sodic phosphate. These experiments showed conclusively that alkalinity alone cannot explain the rise in pressure. Sodium carbonate, for example, which is a much more strongly alkaline salt than is sodium bicarbonate, invariably gave a pronouncedly less marked rise of pressure. The basic sodic phosphate in most of our experiments gave no rise of pressure (this basic salt is, in reality, only very slightly alkaline), and when a rise did occur following its use, we were unable to determine positively that this rise was not coincidental, rather than a causal result. The various solutions of sodic hydrate never caused a rise. The tribasic sodic phosphate, which is a strongly alkaline salt, caused only the slightest rise, and this rise

was immediately followed by a fall, which in two instances ushered in death.

On the evidence at our command therefore we are obliged to conclude that the pressor effect of sodium bicarbonate does not depend solely upon the alkalinity of this salt. This conclusion is important in its bearing on the possible relationship between shock and acidosis, recently emphasized by Henderson. The problem of neutralization of acid by alkali substances in the blood, is very far from being as simple a problem as it is *in vitro,* as the following experiment demonstrates. By the introduction into the circulation, gradually, of a normal solution of acid sodic phosphate ($NaHPO_4$) blood-pressure may be influenced just as it is by shock, i. e., it falls. If not too much of the salt is introduced, the pressure rises again spontaneously, just as it does in a shocked dog that has not been too much compromised. By repeated small injections the pressure may be made to reach a stage where spontaneous recovery does not occur, and where, to all intents and purposes (at least so far as manometric readings go), the animal is in a state of rather profound shock. The analogy between shock and acidosis seems, on the face of it, to be very strong from this experiment. This analogy is further strengthened by the fact that the injection of a small amount of sodium bicarbonate immediately restores the blood pressure to a marked degree. Spiro[4] first outlined this experiment as an example of alkaline neutralization of an acid intoxication and Jacoby[5] considers the experiment as an evidence of "entgiftung" or neutralization of poisonous acid salt by an alkali. We were able to confirm, in detail, the experiments of Spiro, but additional experiments performed by us did not point conclusively to the fact that the rise of pressure following the infusion of sodium bicarbonate was solely due to the alkalinity of this salt. For example, if acidosis was in-

duced by infusing the acid sodic phosphate until a steady low blood-pressure was reached, the pressure could not be raised by instilling ¼ to 1 per cent solutions of sodic hydrate; the introduction of the basic sodic phosphate also had practically no effect, and the introduction of the tribasic sodic phosphate was followed by a rise of pressure not comparable either in degree or in duration to that secured by injecting sodic bicarbonate. Of course, this phase of the problem is fogged by the element of the toxicity of both sodic hydrate and the tribasic phosphate; but, nevertheless, we feel safe in assuming that in these acidosis experiments the pressor effect of sodic bicarbonate cannot unquestioningly be referred to its alkalinity alone.

At this point we were forced, in spite of our fears of gas emboli, to test out the possible blood-pressure raising effect of carbon dioxide gas supplied directly to the blood in shock. Our first experiment demonstrated that we could, with safety, introduce CO_2 directly into the femoral vein from a Kipp generator. A detailed study of the physiological effects of the intravascular injection of CO_2 will be made in a subsequent report. It will suffice here merely to say that by properly regulating the flow of gas, we could administer it over an indefinite period of time, and that although the administration stimulated the respiratory function most actively, it did not influence the rate of heart beat, amplitude of pulse-pressure or height of blood-pressure. When the CO_2 gas was administered before shock was instituted, it had no influence in warding off the oncoming fall of pressure and when administered during shock, it likewise had no influence in restoring pressure toward a normal level. It would seem to be clearly established therefore both that the pressor effect of sodium bicarbonate does not reside in the CO_2 radical, and that shock itself cannot be referred to acapnia.

The direct vasomotor effect of the sodium bicarbonate solution upon the blood-vessels, we have not tested out. Howell (*loc. cit.*), in his work upon sodium carbonate (a salt which, as we have seen, is closely allied to the bicarbonate in its action), says: "It is impossible from the experiments, to state positively whether or not the alkaline injections had any effect upon the tone of the peripheral arteries but the impression that I have obtained from a study of the records is that they act solely as a stimulant upon the heart and that the increased arterial pressure was due chiefly, if not entirely, to a more vigorous heart-beat."

Since none of the factors of bulk, hypertonicity, alkalinity, or free carbon dioxide gas showed itself, the *sole* cause of the pressor effect of sodium bicarbonate, we were forced, by exclusion, to assume that this salt acts specifically upon the heart-muscle. This assumption receives rather strong corroboration from the fact that with both vagi cut, and even with all the higher cerebral centers destroyed by Jackson's[6] method, an injection of sodium bicarbonate is followed by a rise of blood-pressure.

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DISCUSSION

Dr. H. G. Wetherill (Denver, Colo.): Dr. Seelig has done me the honor to ask me to open this discussion, but I must confess that I do not feel at all competent to discuss his paper in-

telligently. Dr. Seelig's work is so advanced and original that about all we can do is to follow along the way he points out.

If the therapeutic points which these experiments suggest have a practical value found for them this must be regarded as a very important contribution. There are several things that seem difficult for us to comprehend, and many that must be taken into account in reaching conclusions. In the first place, in making deductions from such experiments we must differentiate between the effects of the salt of sodium in solution and the effect of the fluid which is used to carry the salt in solution. We know, for example, that the effect of increasing the amount of fluid in the vessels is important in overcoming the retention of blood, practically thrown out of the circulation, through its stagnation in the large venous sinuses of the abdominal viscera.

In closing let me say that I wish to thank Dr. Seelig for this interesting paper. I hope he may be able to work out his data to a practical application so that a new remedy of value may become available for the actual treatment of cases.

Dr. Martin H. Fischer (University of Cincinnati), being asked to take part in the discussion, said: I am much embarrassed in being asked to talk before this body of surgeons. I have been much interested in Dr. Seelig's talk, for we are already indebted to him and G. P. Lyon for a fundamental contribution to the question of surgical shock. These authors have proved that the blood-vessels are not relaxed in shock, as has been generally taught, but are contracted.

The question of shock is a long story, and views regarding its nature differ with every author. However unsatisfactory, I feel that each of us must work out a coördinated scheme of the mechanism by which shock comes to pass, otherwise, when confronted with a clinical case, we must act empirically. Personally, I find myself following the teachings of Yandell Henderson, whose fundamental contributions to this subject constitute one of the brilliant examples of American medicine. His is a theory of shock that gets away from vitalistic explanations and gives a coördinated interpretation of the clinical and experimental observations in shock in the plain terms of physics, chemistry, and physiology. Yandell Henderson has proposed in shock that we give patients carbon dioxide to inhale. Instead of using oxygen pure, he uses a mixture of carbon dioxide and oxygen.

Yandell Henderson discusses two types of shock, the so-called acapnial, or respiratory, and vascular shock. The former can

be relieved by giving or conserving the natural production of carbon dioxide; the second he holds to result from the first. Through interference with respiration he believes a state or lack of oxygen to be produced, and secondarily to this an abnormal acid-production in all the tissues of the body, which, increasing the capacity of the colloids here for holding water, results in sucking water out of the blood-vessels into the tissues. In other words, there is a diminution in the volume of the circulating blood. This second type of shock is called vascular shock, and when sufficiently severe cannot be recovered from. Personally, I have always felt it would have been more correct to say that even the initial acapnia was rather the consequence than the beginning of shock. To me the primary thing has seemed to be an abnormal production or accumulation of acids in the tissues of the body. These acids, being stronger than carbonic acid, would replace this, in consequence of which carbonic acid is thrown off from the blood. The acapnia, in other words, is secondary to the production of the acids in the body tissues. On this basis it becomes easy to explain why it is difficult at times to restore an animal by giving him carbon dioxide to breathe. On the basis of an initial abnormal production of acid in some or all of the organs of the animal or patient, in consequence of pain, injury, excessive muscular work, anesthetic, lack of oxygen, etc., we can easily explain the various signs and symptoms of shock,—the acapnia, the cessation of secretions, the rapid heart, the oligemia, the contracted blood-vessels, etc. As the acid concentration goes up, the blood-vessels go through a series of primary and secondary changes. First, there is a contraction of the musculature of the blood-vessels, but this goes only to a certain point, when relaxation of the blood-vessels begins, and we get a terminal fall in blood-pressure, which is synonymous with oncoming death. We can prove this double effect of progressive increase in acid-concentration on excised blood-vessels and other types of musculature, which will first contract and then relax as we increase the acid-concentration of the medium surrounding them. As some here know, I do not believe that the laws of osmotic pressure play an important rôle in the regulation of the normal and abnormal water content of the tissues, but believe, instead, that all the water of the body is held in combination with colloids of the tissues. When we have an abnormally strong acid and certain other substances produced in the tissues, this capacity of the colloids for holding water is increased; in other words, an edema results. I cannot get away from the belief that the good effects of sodium bicarbonate in shock are due to two things.

With sodium bicarbonate we neutralize, first of all, the effect of the acids on the tissues. This makes them unable to hold as much water as formerly. Not only do they therefore stop sucking water out of the blood, but a current of water starts back into the blood-vessels. The blood-pressure therefore rises, the heart-rate goes down, and secretion begins again. Second, all salts produce the effect of any acid upon a tissue. It is for these reasons that Hogan and I have recommended the use of a hypertonic salt solution to which alkali (sodium carbonate) has been added for shock. Such a solution gives better and more lasting results than does a pure "physiological" salt solution as ordinarily employed, just as sodium bicarbonate is better than a physiological salt solution, as Dr. Seelig has just told us. In spite of Dr. Seelig's contrary views, I still believe that sodium bicarbonate has this double effect, an alkaline effect plus the effects of the salts in reducing colloidal swelling in the tissues. If we can stop the acid effect in the tissues we shall get fluid back into the blood-vessels and so tend to prevent terminal shock. But let us not forget that no salt solution can stay in a blood-vessel. All the water found in the blood, as the water in any tissue, is held in combination with the colloids present in the blood. This is why we do not suck all the water out of the blood into the tissues, or void all our blood and lymph as urine. Free water cannot long remain in the blood- or lymph-vessels. A salt solution can, therefore, act only temporarily. If we want the water to remain in the blood-vessels it must be injected in a combined form, as is the water found in the blood. To this end, Hogan and I have recommended that after the use of alkali and hyper-tonic salts, if the patient again sinks, water in combination with colloids be used. In properly selected cases, we may therefore resort to the transfusion of blood; next best to blood is blood-serum from human sources; next to this foreign-blood serum, as horse serum; and, finally, one per cent gelatin solutions in combination with the proper salts.

Dr. Seelig (closing the discussion): I am indebted both to Dr. Wetherill and Dr. Fischer for their discussions. I did not mean to imply that Dr. Henderson did not try to inject a supply of carbon dioxide gas, but the fact of the matter is he supplied it in two ways, one in salt solution, which does not meet the demands of accurate experimentation, and, secondly, by increasing what he calls the dead-air space of the lung. Unfortunately, he does not supply us with analyses of the lung gases either before or after the experiments, and I feel warranted in

offering my criticism on the basis of the criticism made by the members of the American Physiological Association, who took the stand that Henderson's work would not stand the test of criticism.

There were two things I avoided mentioning, one was the clinical side of sodium bicarbonate, and I avoided that in the hope that at some future meeting some surgeon would report upon the use of sodium bicarbonate. I made this a purely academic paper, in the hope that somebody else would do, as I intend to do, i. e., try out sodium bicarbonate clinically.

The other phase of the subject which I left out of consideration was the question of acidosis, brought up by Dr. Fischer. It seems to me that acidosis as it comes into our clinical lives as an entity is a delusion and a snare. It lends itself easily to laboratory work, but I have had great difficulty in interpreting exactly what it means. I will say for the benefit of Dr. Fischer, that I can take a dog and inject acid sodic phosphate into his vein and thus induce the so-called condition of acidosis. This condition of acidosis resembles exactly the condition of shock. When a dog starts out with high blood-pressure, by injecting acid sodic phosphate it will go down farther and farther, and finally stay down, just as it does in shock.

I gained the impression from my work that sodium bicarbonate would some day come into clinical use, that it would displace adrenalin, and that it would be a regularly established operating-room drug. Whether that impression is going to be borne out or not, I do not know. I did not want my enthusiasm regarding this remedy to go too far, consequently I did not mention the clinical phase of this laboratory problem.

OSTEITIS FIBROSA CYSTICA: REPORT OF A CASE, WITH LANTERN DEMONSTRATION

James F. Percy, M. D.

GALESBURG, ILLINOIS

Miss H. M., aged 23, born of Swedish parents in the State of Nebraska, presented herself for examination September 20, 1911.

History.—She was one of nine children. Her family history is negative except that an aunt died of pulmonary tuberculosis, and the subject of this sketch visited her frequently. Personal medical history, excellent, except whooping-cough, measles and chicken-pox with no sequelæ.

When six years of age, she fell out of a farm wagon backwards, and the left thigh was run over by a corn-planter lengthwise. The right side of the body was not injured. There were severe lacerations of scalp at the vertex. She was in bed only a few days from these injuries. The scar on the right side of chin is due to a lacerated wound received when eight years of age while sliding down stairs. When six years of age, she received another injury by falling off a wire fence and was caught by one of her feet. This wrenched one of her thighs, but she cannot remember which one it was. When three years of age had a "running sore" of unknown origin on the outer side of the right heel. She did not wear shoes for over three months. The scar resulting from this lesion is nearly an inch long and half an inch wide. Menstrual history, normal. No rheumatism or arthritis.

History of present condition for which she seeks relief: When twelve or thirteen years of age (1889) she began to be lame in the left hip. It was unaccompanied by pain, but sometimes a soreness of the muscles of the thigh on that side was referred to. The rather slight limitation of function at first, as expressed by limping, was quite easily overcome when her parents reproved her for it. The lameness was slowly progressive and caused no special pain until 1903. If she walked more than usual or slipped, the pain became more acute. During the last five or six years she could not run or do much walking, and there has been more or less pain in the hip all of the time, whether resting or not.

Present Status, December 13, 1912: Weight, 117.5 pounds, temperature, normal; hemoglobin, 85 per cent; blood-pressure, 120 (systolic); lungs, negative; heart, mitral regurgitant murmur with fine compensation; abdomen, negative; right thigh, normal. Measurements: right side from anterior spine-ilium to internal malleolus, 83.5 cm.; left, 77.5 cm. Left trochanter, 4.5 cm. above Nelaton's line. Region of left trochanter, noticeably enlarged. Left lateral curvature of spine, middle dorsal. Left thigh smaller than right, at junction of upper third with lower two-thirds. Measurements: 44.5 cm.; right, 46 cm.; above ankles, left 18.5 cm.; right 19.5 cm. Patient walks as if she had congenital dislocation of left hip. Wassermann, negative.

Diagnosis.—Coxa vara.

The *x*-ray plates herewith presented necessitated, as it so often does when made use of in bone-lesions, a revision of the diagnosis to that found in the title of this paper.

Treatment.—The patient was put to bed, and extention of the thigh attempted by heavy weights with

Fig. 1. Left femur. The x-ray picture that made the diagnosis. The coxa vara position is well shown. The shaded map-like lines indicate the location of the trabeculæ that serve to strengthen the bone-cavity and also act as containers for the cyst substance. A cyst-pocket is shown in the lower end of the body of the ilium, just above the acetabulum.

Fig. 2. The multiple cysts of the left ilium. These were recently curetted and the cavity filled with von Mosteg's bone-plug.

Fig. 3. Shows cysts of the head of the right femur. No deformity.

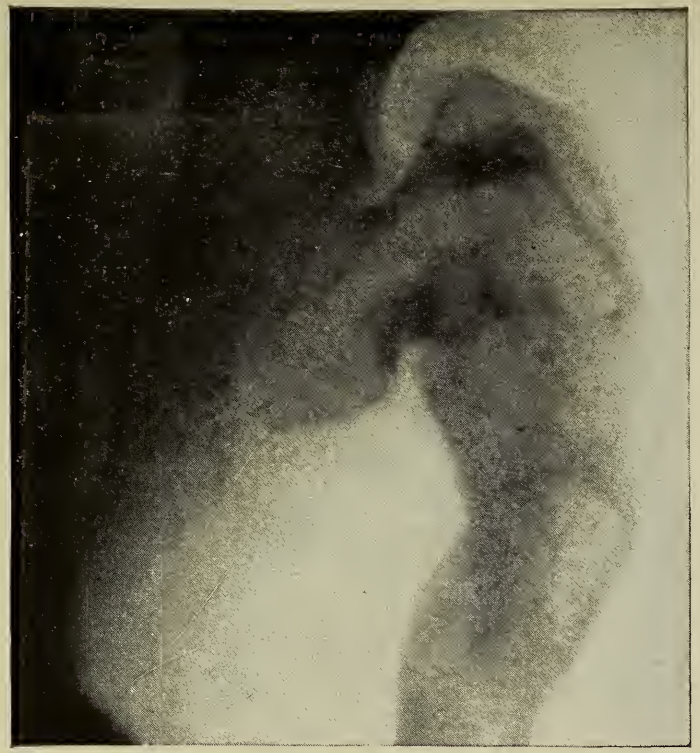

Fig. 4. Fourteen weeks after curettement of the head of the left femur.

the hope that the long-existing shortening of the fascia and muscles would be overcome sufficiently to bring the limb down to its normal position. As the *x*-ray pictures show, such an attempt would seem indicated because of the extreme attenuation of the upper end of the femur in this case. Nothing, however, was accomplished as far as lengthening the limb was concerned after six weeks of active effort with the weight and pulley. Under ether the joint was manipulated with the idea of stretching the capsule, fascia, and muscles, and the weights reapplied for another two weeks, but again nothing was accomplished.

Operation December 4, 1911, at the Galesburg Hospital: Longitudinal incision extending above the great trochanter and below sufficiently to give access to the pathological tissue. It was found that the periosteal covering of the bone, only, was not involved in the disease. This was easily broken through with the knife, exposing a substance beneath that reminded one of the soft cocoanut candy to be purchased in the shops, mixed with putty. When this was removed it was found by a thorough use of the curette that it was contained in cavities which averaged in size that of a large olive. The mass was cohesive, even elastic enough to be rolled out with the aid of the curette without fear of disintegration.

The trabeculæ of the cavity were thin, very smooth, hard, and translucent. As already stated, the periosteal covering was thin and easily penetrated with the knife or curette. The partition walls were but few in number, very dense, and arranged in such fashion as to suggest an attempt to strengthen the collapsing trochanter, neck, and head of the bone. This description will also apply to the head of the bone in the acetabulum. There was a large quantity of this rubber or putty-like mass. When removed, it would easily have filled the ordinary breakfast coffee-cup. One

interesting fact incident to the emptying of the cavity by the curette was the practical absence of hemorrhage. Nothing further of special moment occurred during the operation except that it was followed by a surprising amount of profound shock.

The thigh was encased in plaster of Paris extending from the breasts to the toes and sharply abducted with the hope of overcoming, as far possible, the coxa vara position of the neck. As will be seen (Fig. 4) by the *x*-ray taken fourteen weeks after the operation, nothing has been accomplished by this position. The patient was in the hospital eleven weeks. She wore the plaster spica twenty-nine weeks. She had used crutches until recently and still uses a cane in walking.

The operation has relieved her of all pain and given her a sense of security in using the joint that was not present previous to the removal of the disease process. As shown by the *x*-ray of this patient's left ilium, this bone (Fig 1) is also extensively involved in the disease. The head of the right femur, as shown by Fig. 2, is involved in the disease, but no signs of deformity have yet appeared. The disease here, and in the right (Fig 3) and left ilium, will be removed by operation at the first opportunity. *X*-ray pictures of the whole body failed to discover other bones involved in the cystic process.

It gives the writer of this paper much satisfaction to be able to present to this Association the report of a case suffering with a condition that so far has been observed but rarely in this country. It is his belief that the disease is not so uncommon as its failure of mention in the surgical literature of the English-speaking world would indicate. It is with the hope that the presentation of this paper, together with the *x*-ray plates taken from the case here reported, will excite

Fig. 5. Cysts of the right ilium.

Fig. 6. A, islet of bone tissue. B, layer of cells rich in osteoblasts. C, layer of cells containing osteoclasts. D, connective-tissue cells.

interest in a most fascinating pathological and surgical subject.

Osteitis fibrosa cystica was first described by von Recklinghausen in 1891* in the Festschrift for Virchow given in that year as consisting of single and, at times, multiple cysts. Pfeiffer†, whose paper covers the literature of the subject extensively and thoroughly, collected 49 reported cases. Of these, 19 occurred in the femur, and in 14 of this number the disease was in the upper end of the bone. Twelve were found in the tibia, and 9 of this number were also in the upmost end. Of the humerus, 10 cases are given, with 8 at the shoulder. Other bones to be affected were the radius, ulna, fibula, clavicle, calcaneus, metatarsus, metacarpus, phalanges, and skull.

Von Recklinghausen makes an interesting statement in his first paper, namely, that many tumors of bone, formerly classed as sarcoma, are undoubtedly only types of osteitis fibrosa cystica. In a more recent paper (1911) he tells us, in addition to the above, that most cases of cysts of bone are osteitis fibrosa. From the literature we learn that M. B. Schmidt, Mönckeberg, Glimm, Tietze, Pfeiffer, and Rehn hold similar views. The conception of these authors, as to the origin of these cysts as a distinct entity and separate from sarcoma, gives added interest to the reading of the paper by Bloodgood of Baltimore on "The Conservative Treatment of Giant-Cell Sarcoma, with the Study of Bone-Transplantation" in the *Annals of Surgery* for August, of this year (1912).

Bone cysts are found in tuberculosis, osteomyelitis, osteomalacia, sarcoma, myxoma, and chrondoma. Hydatid and maxillary cysts should also be considered in this category.

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*Von Recklinghausen: Festschrift der Assistenten für Virchow zu seinem 71 Geburtstage, Berlin, 1891.

†Pfeiffer: Ueber die Ostitis Fibrosa und Genese und Therapie der Knochensystem, Beiträge zur klin. Chir., Bd. liii, H. 2.

Pfeiffer gives the age incidence in his collected cases as follows:

1—10 years	6 cases
10—20 years	29 cases
20—30 years	7 cases
30—40 years	4 cases
Above 40 years	3 cases

Von Mikulicz holds that the disease, first, is one found during the growing period of life; second, that it is a disease of the long bones; third, it is associated with an initial trauma; and, fourth, that it is a distinct process.

There are four theories as to the etiology of these cysts:

First, that of von Recklinghausen, who explains the disease on the basis of a mechanical process, perhaps following a trauma, and he is sustained in this by Pfeiffer, who points out that the disease is found most often in those bones and parts of bones which sustain the brunt of greatest activity in life through pull, weight, or pressure.

The second theory attributes the presence of these cysts to an infectious process. Both Lubarsch and Rapke, working separately, found organisms in the contents of the cyst cavity.

Third, Boit believes that the mechanical theory of von Recklinghausen and Pfeiffer is not sufficient to explain the development of osteitis fibrosa cystica in all its forms. He would rather believe that the causes are various, viz., toxic, metabolic, traumatic, infective, or some form of abnormal internal secretory change. The fourth theory is that of Eduard Rehn, who, in a most thoughtful article*, suggests, in contradistinction

*"Die Schnüffelkrankheit des Schweines und ihre Beziehungen zur Ostitis fibrosa infantilis des Menschen," von Dr. Eduard Rehn. Beitrage zur pathologischen Anatomie und zur allgemeinen Pathologie, vol. 44, 1908, p. 274.

to the theory of von Recklinghausen and Pfeiffer, that the disease occurs not in those bones which, through pull or weight or pressure, are the most used, but in those bones and those parts of bones which are most involved in growth. This author considers osteitis fibrosa and the snuffle disease of hogs (die Schnüffelkrankheit des Schweines) identical, and believes both of them to be an overdevelopment of a normal physiological process. By a most interesting course of reasoning, he states that osteitis fibrosa is not an entity, but a phase of dystrophy found in callus, rickets, osteomalacia, sarcoma, senile osteoporosis, Barlow's disease, etc., and therefore the diseases should be known as osteodystrophy malacia, scorbutica, deformans, cystica, and the last disease (identical with osteitis fibrosa) should be divided into two groups, osteodystrophy deformans infantilis and osteodystrophy deformans senilis. The snuffle disease of swine belongs to the first group.

Boit*, in an interesting comparative study, teaches that the Leontiasis ossa of Virchow and the osteitis fibrosa of von Recklinghausen are histologically identical. To the above may be added the interesting studies of Eduard Rehn, which show the snuffle disease found in swine to be identical with osteitis of men.

Bockenheimer† states that Paget's osteitis deformans and osteitis fibrosa are identical, pathologically.

Relative to the pathology of osteitis fibrosa, it is well to call attention to the fact that cyst-formation in tumors is the result of softening in the center, whereas is osteitis fibrosa it results from a lacuna formation following a shrinkage in the surrounding soft tissues.

Pathology: The disease consists of multiple or single cysts occurring chiefly at the metaphyses of long

*Boit: Ueber Leontiasis Ossea und Ostitis Fibrosa Arch. für klin. Chirur., vol. xcvii, p. 515.

†Quoted by Boit.

bones causing them to become fragile, porous, and easily bended. The process consists of an exaggeration of normal physiological growth with a proliferation of osteoid tissue in the formation of newly found small lamellæ of bone with a spongy absorption brought about through the agency of osteoclasts. The tissues surrounding the islets of bone are rich in round osteoblastic characteristic cell-elements, at the outskirts of which may be found a number of osteoclastic cells. Between these islets, which give the tissue the appearance of a giant-celled sarcoma, a number of spindle-formed fibrous-tissue elements are found. Numerous vessels are scattered throughout the tissue, and here and there a recent hemorrhage may be observed. The periosteum is rich in osteoblastic elements, but is otherwise normal. (Plate 5.)

The symptoms are those of a very gradual and increasing loss of normal function extending over a period of years. Pain also is not pronounced until the part involved has first felt the discomfort incident to the loss of function. In the beginning it is usually spoken of as an aching or soreness or stiffness or even neuralgia. After unusual or excessive use the bone involved is actually the seat of distressing discomfort, which persists until the part has been given a period of rest. When the disease is in the upper end of the femur the loss of function is not unlike that which is characteristic of coxa vara. The walking-gait reminds one of that seen in cases of congenital dislocation of the hip.

The course of the disease is persistently chronic. The osteoporosis, due to the persevering destructive action of the osteoclastic cells, weakens the bony framework to the point of deformity, suffering, and uselessness. It is this that explains the frequency of pathological fracture, which, according to Pfeiffer, occurred twenty times in his report of forty-nine cases. Curi-

ously, this author advises against operative measures because the disease is essentially not malignant. He would use the knife only for the purposes of diagnosis or where the pain is excessive. Von Mickulicz recommends injections of iodoform emulsion. Boit, however, insists upon radical treatment.

The case here reported has been greatly relieved by the operative procedures followed. The pain is gone, and the sense of insecurity and weakness in and about the hip have disappeared. True, the deformity has not been overcome, but this can be left to subsequent surgical treatment if it seems wise to attempt it. The most important problem, it would seem to the writer, is the method to be pursued in this case where the iliac bone, especially the right (Fig 3), is so extensively involved. This question must also arise when the disease occurs in the small bones of the foot and hand or in the skull. It is necessary to remember that the disease always involves the endosteum and not the periosteum, and this fundamental pathological fact must be the basis of our interference to arrest destructive progress.

The indications for operation can safely be enumerated as follows:

First, interference with function and the development of deformity. Second, pressure upon important vessels, nerves, and organs. Third, prevention of pathological fracture from a large osteoporosis resulting from the disease. When this occurs, operation is contra-indicated as a process of repair seems to be immediately initiated. It would appear to the writer that when the pernicious character of the pathology is considered, especially in relation to the ruinous work of the osteoblastic cells, a permanent recovery can be obtained in no way other than by radical operation.

Dr. Dean D. Lewis (Chicago): Through the kindness of Dr. Percy I have had the opportunity of examining the tissue removed from this case. Histologically, the tissue presents all of the features associated with fibrous osteitis, which, I believe, is more common than was formerly thought to be the case. Clinically, I believe that we must recognize two forms of fibrous osteitis, the diffuse and the localized. Cases are not uncommon in which the disease affects the metaphysis of one of the long bones and remains localized, the lesion repairing after a pathological fracture or removal of the fibrous tissue by curetting. There are other cases in which a number of bones become involved successively, the clinical course of the disease extending over a number of years, death from cachexia finally occurring.

It seems to me that the *x*-ray examination in these cases is most important, and I believe that in most cases a positive diagnosis can be made by the *x*-ray findings alone. The slow clinical course of some of these cases may be a determining factor in making the diagnosis. The location of the lesion in the metaphysis of the bone, the perfectly symmetrical expansion of the cortex of the bone until it becomes of egg-shell thinness, the failure of extension of the pathological process to the surrounding soft tissues even after pathological fracture has occurred, and the presence of trabeculæ dividing off definite loculi, are quite characteristic. The *x*-ray findings are indeed so positive in most cases that one need have little difficulty in differentiating fibrous osteitis from giant-cell sarcomas.

We are still in the dark regarding the etiology of fibrous osteitis. There are but few cases in which organisms have been found, and in those in which organisms have been found it is doubtful whether they should be regarded as the essential etiologic factor. Comparative pathology seems to throw some light on this, for "snuffles disease," occurring in pigs has the same bone-changes as are found in fibrous osteitis in man. This disease in pigs may be secondary to teeth-infections.

Surgeons should emphasize the importance of recognizing fibrous osteitis, for I believe that mutilating operations upon the extremities have been performed in some of these cases, the lesion being considered malignant. Fibrous osteitis can be cured by curettage of the bone-cavity. In some cases resection of the diseased bone may be required, but this operation will be performed less frequently when the lesion is recognized early. I do not believe that the lesion should be operated upon when spontaneous fractures occur. The fracture should be reduced and

maintained in position. The bone tends to repair in these cases, and the reaction incident to the fracture starts reparative processes, which may result in a healing of the pathological process.

Dr. Joseph F. Smith (Wausau, Wis.): I would like to ask Dr. Lewis in this connection about a case I saw recently. The *x*-ray picture presented the typical appearance of bone-cyst of the lower jaw. The macroscopic appearance of the tissue was altogether similar to that Dr. Percy described and the microscopic picture shown by Dr. Percy resembles that in the tissues removed in my case. In my case, however, there was a non-erupted tooth at the bottom of the cavity. The cavity had a thin will, and in my judgment the tooth was merely an incident, and probably had nothing to do with the development of the cyst. It did not seem to be a true cyst of the dental organ.

Dr. L. L. McArthur (Chicago): I would like to say a few words on this subject. I was down on the program for a paper at the meeting of the American Surgical Association this year, but gave way to Carrel. The title of my paper was the same as that of Dr. Percy. I had three cases to present, one of which was a case of tumor of the mental prominence of the lower jaw in a young girl. This, under the *x*-ray eight years ago, gave the characteristics of sarcoma. Operative interference at the time revealed a shell-like covering of a cartilaginous-like material lining the tumor cavity, with prominence of the jaw. This cartilaginous-like cavity was of such a density that it required a gouge to cut it, and it resembled almost the yellow cartilage of the ribs. The pathologist made frozen sections at the time and pronounced it a giant-cell sarcoma, and stated he thought the jaw ought to be resected. So different was it from all giant-cell sarcomata I had ever seen of bone, that I refrained from resecting the jaw and simply curetted out the cavity. The wound healed, and I waited seven years before rejoining the two loose fragments, which had during these seven years been held together by a dental prosthetic device. At the end of seven years the same pathologists were present when I made an operation for the introduction of a graft to bridge the gap. Strangely enough, a small fragment of the same type of growth was found, and this time it was pronounced a bone-cyst. Although the slides are identical, it illustrates the difficulty of determining between bone-cyst and giant-cell sarcoma from the microscopic slide. I might say, afterwards the visible deformity of the neck was reduced by implanting a free graft of fat after bony union had occurred.

The second case was one of the neck of the femur in a little girl in whom spontaneous fracture had occurred. While running at play she fell and sustained a fracture. The *x*-ray showed the characteristic picture seen here today of a porous condition of the bone. This case was under the care of Dr. Carl Beck and was operated upon by him. A cystic cavity was recognized as a bone-cyst, and surgical treatment recommended of curetting the cavity. Unfortunately, it did not clear up completely. He did not remove the entire area of bone-cyst formation. Later the case drifted to me. I again operated and this time found that the neck was separated from the head of the bone, perfectly loose from the trochanter, so that they could be easily lifted out. I freshened the inner aspect of the trochanter obliquely and put it in somewhat the Lorenz position against the head which floated in the socket with flat surface at the line of epiphysial cartilage junction as a compass dial floats. Strangely enough, I secured bony union between that head and this trochanter. The child runs around, plays, and is quite well, aside from the necessary shortening.

The third case resembled somewhat the picture presented by Dr. Percy, except that the femur assumed a curve including the neck of the femur and great trochanter and a portion of the shaft like the curve of that larger one shown by Dr. Percy (indicating). As seen under the *x*-ray picture, there was an enormous bow to the upper shaft of the great trochanter and neck, but hollowed out with a cystic-like cavity. This child had three times broken her leg, was laid up for six weks, ran around for a month or two, then broke her leg again. The third time she was admitted to the service at St. Luke's Hospital. An *x*-ray showed a bone-cyst cavity involving the neck and upper two and a half inches of the shaft. In this case I again endeavored to clean out the cavity, and did what I had not done before, nor seen recommended, namely, introduced some Senn bone-chips. These healed in nicely. We secured primary union, and that child, two and a half years afterwards, is running around. She has had no subsequent fractures. As Dr. Lewis has said, there is probably a low-grade infection. If you reach healthy bone, you will secure union. We can implant in such a cavity a graft of living bone, as was done in the jaw case, and get osseous union, or we can implant chips in large cavities that will grow like the Senn bone-chips.

HOW MUCH PATHOLOGY MAY THE GALL-BLADDER HAVE AND FUNCTIONATE?

Byron B. Davis, M. D.

OMAHA, NEBRASKA

What are the exact functions of the gall-bladder? According to all the text-books on physiology with which I am acquainted its function is that of a "reservoir for the bile." Its normal capacity being from an ounce to an ounce and a quarter and the daily secretion of bile varying from twenty to forty ounces its value as a storage-reservoir cannot be very great.

With no desire to be considered irreverent I cannot avoid the feeling that this time-honored statement is ridiculous. It certainly seems to serve as an aid in accelerating the flow of the bile towards the duodenum, and does this by a method somewhat analogous to the action of the governor on a steam-engine. By a firm muscular contraction it expels a large part of its contents by driving the bile through the cystic duct along the common duct into the duodenum. During its systole the column of bile in the hepatic ducts is forced to stand still. As soon as the contraction is over a period of relaxation ensues, the gall-bladder returning to its normal size and capacity. This produces a tendency to the formation of a vacuum, and causes the bile in the hepatic ducts to flow rapidly away from the liver and to refill the gall-bladder, replacing the bile that has just been expelled and perhaps continuing the pressure against the column of bile in the common

duct and causing it to flow into the duodenum, but less rapidly and less forcibly than during the contraction of the gall-bladder. After a short rest the contraction is repeated. The entire systole and diastole of the gall-bladder occupies a period of fifteen to twenty seconds.

There are, besides, a number of other forces that are actively engaged in carrying the bile safely to the duodenum. The first of these is the pressure from behind, due to the freshly formed bile occupying the smallest biliary ducts and increasing the pressure. The diaphragm at each contraction presses down on the liver, squeezing the bile onward. In all probability also a suction force is periodically exerted by the duodenum in the lull between the action of its peristaltic waves.

Granted that this explanation of the function of the gall-bladder is correct, any pathological change that interferes with the free passage of the bile into its cavity or that impedes its ready exit, is inimical to its usefulness. In like manner any pathological condition that destroys the elasticity of the gall-bladder walls, or that takes away its ability to contract and expel its contents, renders it functionless.

With such a functionless organ retained within the body, the patient is in worse condition than if it were not there. Besides being prone to degenerations, as all functionless tissues are, it will act as a mechanical torture. When it becomes distended, with no ability to empty itself, pain will result. If it still possesses power to contract, but cannot empty itself because of obstruction in the cystic duct, there will be colicky pains. Filled with stagnant bile, which is sure to serve as a culture-medium for germs, infections are almost certain to result.

The retention of a gall-bladder that no longer is able to carry on its functions is the cause of much suffering, as is proven by the large number of cases every active

surgeon has encountered who suffer as badly after as before the gall-bladder is drained, and sometimes the condition is even worse.

Certain conditions that are often found at operation are bound to destroy normal function for all time. Undue narrowing or complete obliteration of the lumen of the cystic duct makes a continuation of function of the gall-bladder impossible. If the obstruction is incomplete, bile will slowly flow in, but the process of emptying is so retarded that stagnation, which always invites infection, is sure to occur, sooner or later. Complete obstruction of the cystic duct not only permits no bile to pass into the gall-bladder, but the constant formation of mucus and transudation of serum into the cavity of the gall-bladder insures the existence of a permanent fistula or, what is worse, a recurring mucous fistula, with all the suffering which that entails. The consecutive stages of closure of the fistula, gradual filling and distention of the gall-bladder, intense pain from over-distention, probably an acute infection with high fever, temporarily relieved by reopening the fistula, which is prone to close again, and all this train of distressing symptoms recurring in regular order, are too familiar to all surgeons. The only relief for such a condition is removal of the gall-bladder, which should have been done at the first operation instead of draining.

Changes in the bladder wall, as when it has become thin and fibrous, practically scar-tissue; or is greatly thickened and infiltrated; or when the viscus has become greatly distorted by inflammatory bands and partitions passing across from wall to wall; or when the gall-bladder has become very small and atrophied through successive inflammatory attacks; or when the mucous membrane has been destroyed by infection or by pressure-necrosis, due to contracting down on large stones, will not only destroy its elasticity, but will for-

ever inhibit its power for rhythmical contraction. Such a gall-bladder will fill with bile, which cannot be expelled. It becomes stagnant, more or less precipitation of cholesterin occurs, and it has wonderful capacity for harboring and growing bacteria and setting up infection.

When at the primary operation the gall-bladder is found well surrounded and held by adhesions or is badly denuded of peritoneum, there is nothing that can be done to prevent new adhesions forming. Such an organ held by adhesions cannot contract. It is so held that every effort at contraction by pulling on the adhesions produces pain and fails to expel the bile. Here again is stagnant bile with all its accompanying ills. That this is not fanciful is proven if one thinks of the cases of this character which have been completely relieved by removal of the gall-bladder.

Gangrenous bladders or those with gangrenous areas ought also to be removed, unless the condition is so serious as to make it unnecessarily dangerous, in which case drainage with careful quarantining will have to do temporarily until the patient is in condition to withstand the more radical operation.

Primary cholecystectomy is not recommended for any of the conditions described if the operation is thereby rendered extrahazardous. Such a condition is very rarely encountered. In many cases primary cholecystectomy is as safe as drainage and effects a prompt cure, while drainage in the classes of cases described does not cure, often does not even mitigate the patient's sufferings, but makes it necessary to operate again, when, in all probability, cholecystectomy will be more difficult and more dangerous than it would have been as a primary operation.

Although safe and efficient delivery of the bile through the hepatic and common ducts into the duodenum can be carried on by the other forces without

its aid, it can be done more effectually and with less liability to damming up of the current when accelerated by a perfectly functionating gall-bladder. I would therefore be the last to recommend the indiscriminate removal of this organ. It ought never to be done unless so badly damaged that its function will be permanently lost.

To many it may seem that this subject was settled long ago; but when one meets so many patients whose gall-bladders have been drained, but who are not cured, there seems to be something still to be said. The fact stands out that drainage operations are not followed by the uniformly good result that ought to be obtained. Patients too often feel a regret that they did not keep away from the surgeons. It is my opinion that many of the unfavorable results might be avoided by a more thoughtful consideration of the capacity of the gall-bladder with which one is dealing to carry on its functions.

Careful study of the pathology found at operation, and clear-cut notions of what to expect of a gall-bladder when these several changes have occurred, with good judgment in deciding when to drain and when to extirpate, will go far to improve the results and prevent post-operative disappointment.

DISCUSSION

Dr. Charles H. Mayo (Rochester, Minn.): There are one or two points in connection with this important paper that I wish to speak of. I believe that we remove the gall-bladder very frequently when it should not be removed and *vice versa.* In about one-half of the cases which return for a second operation, the gall-bladder should have been removed primarily. In the other half, the trouble will be found due to an enormous mass of adhesions,—the duodenum scarred and adherent to the liver, the pylorus adherent to the gall-bladder and the margin of the liver, etc. One may not recognize that these adhesions produced the secondary symptoms, that the gall-bladder was not diseased, and that the adhesions might have been prevented by utilizing some of the fat in the manner described by Dr. McArthur.

PYELOTOMY FOR THE REMOVAL OF RENAL CALCULI

Daniel N. Eisendrath, M. D.

CHICAGO, ILLINOIS

In the removal of calculi from the kidney we aim to do as little damage as possible to the secreting structure. The first to suggest removal of calculi through an incision along the convex (outer) border of the kidney was Henry Morris, in 1880. For many years this was the only method in general use and the term "nephrotomy" was applied to it, no matter whether the incision was only large enough to introduce the index-finger or whether the incision bisected the kidney in the same manner as we open it at autopsy. With the development of radiographic technic, a new era began, and the ability to determine accurately the location, form, size, and number of calculi in the kidney made it unnecessary to remove every calculus, no matter what its size or location might be, through a nephrotomy incision. The demand for some other method, especially for smaller calculi located in the *renal* pelvis, became especially urgent when reports of post-operative bleeding and other sequelæ of nephrotomy began to accumulate.

Strange to say, the removal of calculi through an incision into the renal pelvis, technically termed a *pyelotomy*, was suggested by Czerny in the same year that Morris first performed nephrotomy. The operation of pyelotomy was employed rarely, however, until within recent years. French surgeons have been the

most enthusiastic advocates of the method, and thus pyelotomy has gradually again attained the important position, which, in my opinion, it deserves, as the method of choice in the removal of renal calculi. Of late, reports equally favorable to those of the French surgeons have come from the German clinics. In this country many surgeons still cling to the older operation of nephrotomy, and it is with the object of calling attention to the many advantages of pyelotomy over nephrotomy that this paper has been written. In order more fully to understand some of the points to be referred to in the description of the operation, a brief review of some anatomic facts is necessary.

ANATOMY OF THE KIDNEY

The renal vessels, both artery and vein, begin to divide just before reaching the inner or mesial border of the renal pelvis, so that the entire anterior surface of the pelvis is covered by the primary branches of the main artery and vein. These relations are well shown in Figure 1. For this reason an incision into the anterior aspect of the pelvis is impracticable, on account of the intimate relations of so many arterial and venous trunks to this surface. The chief or primary arterial branch supplying the upper pole gives off a fair-sized vessel just before entering the substance of the kidney. This artery winds around the upper border of the pelvis as a retropelvic artery, and, after arching across the posterior surface of the renal pelvis, gives off branches to supply the posterior half of the kidney. This retropelvic artery is quite constant, but, fortunately, it is so well within the hilum and so far away from the incision usually made through the posterior surface as to be practically a negligible factor. It is well, however, to watch for anomalies of this artery and to make the incision not too near the parenchyma of the kidney, i. e., a fair distance away from the junction of

Fig. 1.—Relation of arteries and veins to anterior surface of renal pelvis. (Albarran.)

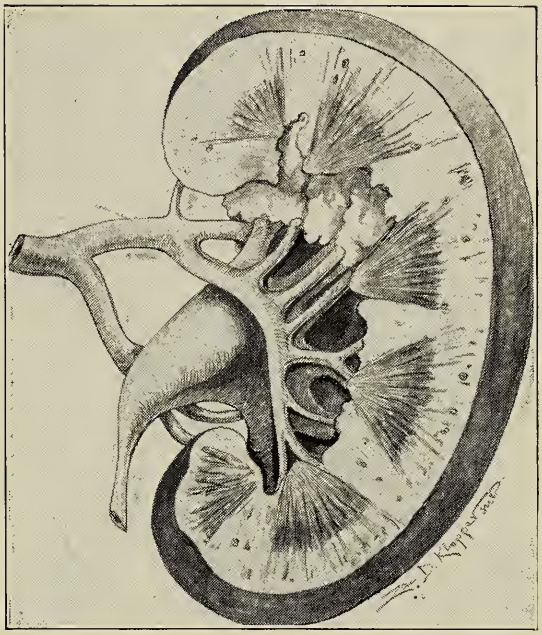

Fig. 2.—Distribution of retropelvic artery. (Albarran.)

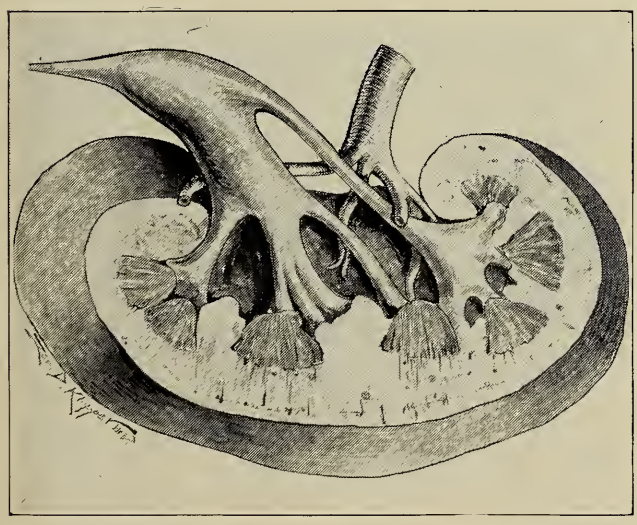

Fig. 3.—Relation of renal vessels to bifid type of pelvis. (Albarran.)

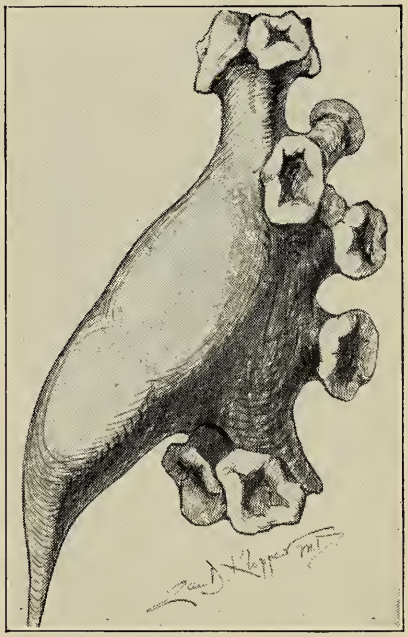

Fig. 4.—Typical ampullary type of renal pelvis. (Albarran.)

Fig. 5.—Various types of renal pelvis. (Albarran.)

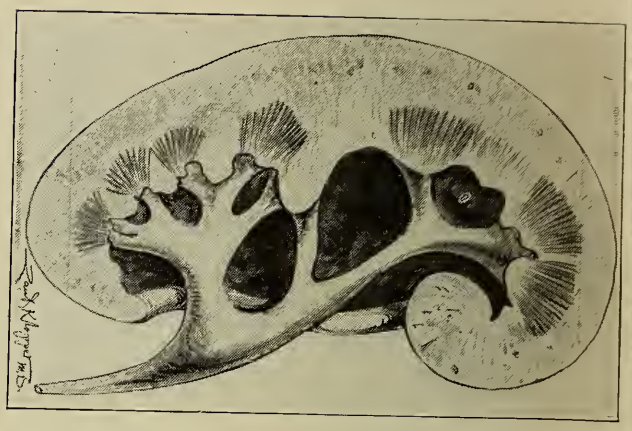

Fig. 6.—Typical bifid type of renal pelvis. Note small size of extrarenal portion. (Albarran.)

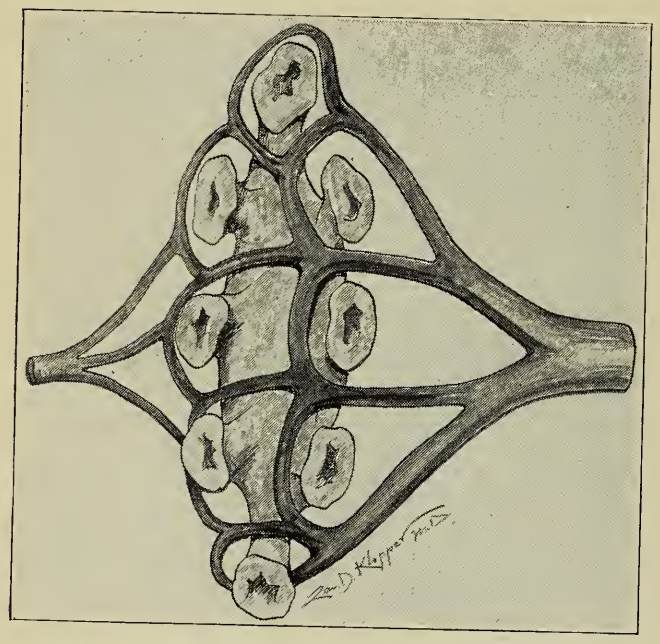

Fig. 7.—Relation of peripelvic veins to papillæ. (Albarran.)

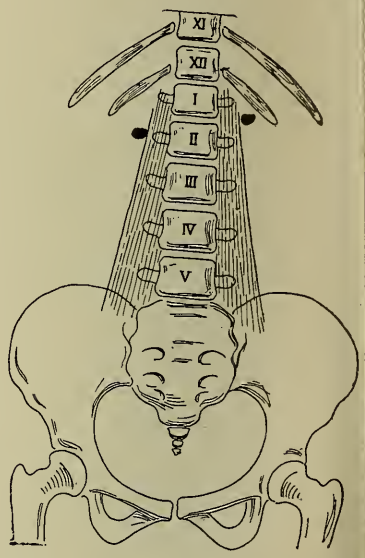

Fig. 8.—Typical location of stones located at junction of ureter and renal pelvis (ureteropelvic junction). Note exceptions in text.

the intrarenal and extrarenal portions of the pelvis. Both the anterior and posterior surfaces of the renal pelvis are covered with a finely lobulated layer of fat. Some small veins are often found in this peripelvic fat layer. When these veins are severed, bleeding is easily controlled by a fine catgut transfixion ligature. Aside from these small vessels and the above-described retropelvic artery, the posterior aspect of the pelvis is by far the least vascular portion.

Types of Pelvis.—In the early history of pyelotomy but little attention was paid to the fact that not all kidneys possessed the same type of pelvis. In order to become proficient in the operation of pyelotomy, an operator must know that a number of different types of renal pelves exist. Although there are eight or nine different forms, it is necessary to bear in mind only the principal two, the ampullary (Figs 2, 4, and 5) and the bifid types (Figs. 3 and 6). In the ampullary the ureter ends in a renal pelvis, which has only a single cavity of fair size, from which short primary calices arise, which receive the papillæ of the parenchyma. A calculus lodged in such a single sac-like pelvis is comparatively easy to extract unless it be tightly wedged in one of the short primary calices. (Fig. 4.)

Conditions are quite different, however, in the bifid (Figs. 5 and 6) or the still more complex trifid type of pelvis. In the bifid form, the renal pelvis proper is small and divides almost immediately into two relatively narrow primary calices which again subdivide in order to receive the papillæ. (Fig. 6.) The frequency with which the various types of pelves occur has never been studied, so far as I am able to learn. In my own experience about 70 per cent of renal pelves have been of the ampullary or single type, and about 20 per cent of the bifid or double type, the remainder being made up of the other varieties. The relations of the renal

vessels to the various types of the renal pelvis remain the same.

Relations of Peripelvic Veins to Primary and Secondary Calices.—A recent experience, referred to later under the technic, has stimulated a study of the relation of the calices into which the renal pelvis divides, to the chief branches of the renal vein. The latter, like the first divisions of the renal artery, lie in close relation to the anterior aspect of the pelvis, both within and without the kidney. Each papilla, moreover (Fig 7), is surrounded by a network of veins which collect the blood returning from the renal parenchyma. For this reason a stone lodged in a narrow secondary calix or branch of the renal pelvis must of necessity lie in close relation to some of these peripelvic veins. One can readily see how pressure atrophy of the only tissue, namely, the pelvic mucous membrane, separating the peripelvic vein from the stone, could occur, and the removal of the calculus be followed by a severe hemorrhage. It cannot be denied that the same accident might occur when the calculus is pulled out through a nephrotomy incision, as when it is loosened and pulled down, be it ever so gently, through the pyelotomy incision.

The relation of the branches of the renal arteries to the outer wall of the pelvis is not less intimate than that of the veins. No case has yet been reported of an arterial hemorrhage following pyelotomy similar to the decubital venous one which I report later in this paper. I mention these vessel relations to the outer aspect of the intrarenal portion of the pelvis because the operation of pyelotomy was at first limited to calculi lying loose in the renal pelvis or moderately tightly impacted in the point of junction of the ureter and renal pelvis. Of late, the sphere of usefulness of pyelotomy has been extended, especially by French surgeons, to the removal of calculi much

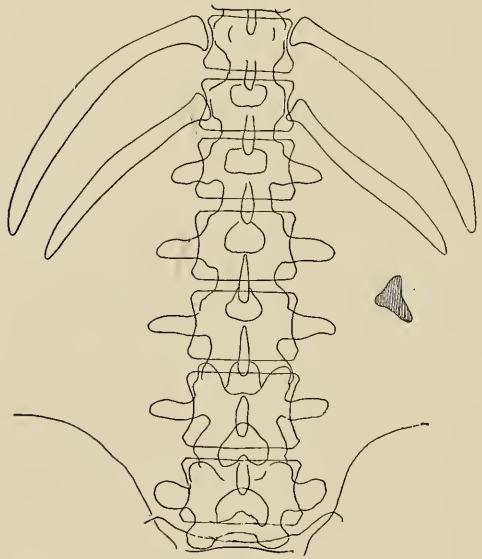

Fig. 9.—Tracing of skiagram of typical nipple-like calculus located at ureteropelvic junction. (Dr. C. E. Kahlke's case.)

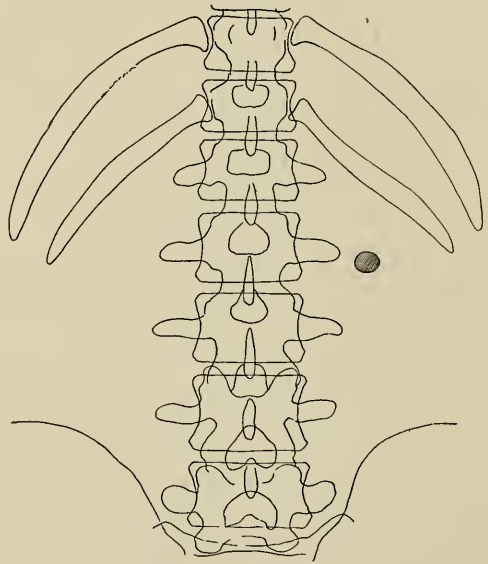

Fig. 10.—Tracing of skiagram of calculus located in upper pole of low-lying kidney. Illustrates exception to statement that shadows at this location are always due to stones at the ureteropelvic junction. (Dr. G. Kolischer's case.)

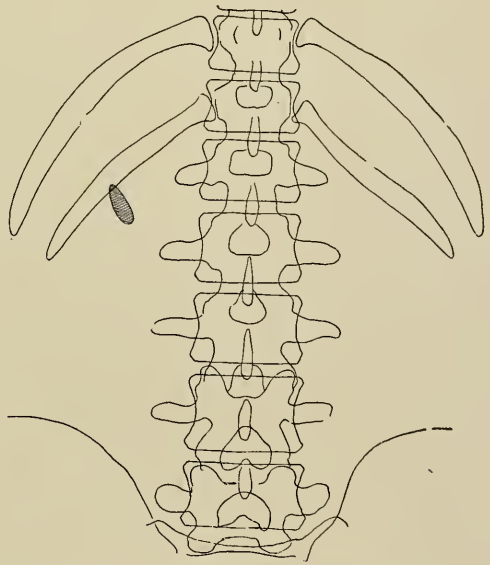

Fig. 11.—Tracing of skiagram of almond-like calculus located in lower pole of high-lying kidney. To illustrate exception to statement that shadows so located are due to calculi at ureteropelvic junction. (Author's case.)

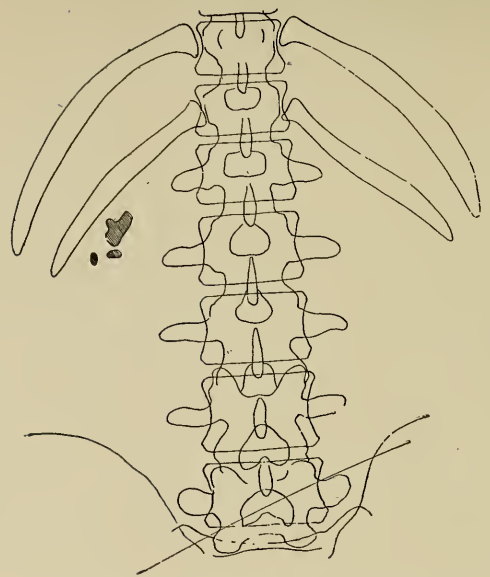

Fig. 12.—Tracing of skiagram of calculi at ureteropelvic junction calix of bifid type of pelvis. Recovered by pyelotomy. (Author's case.)

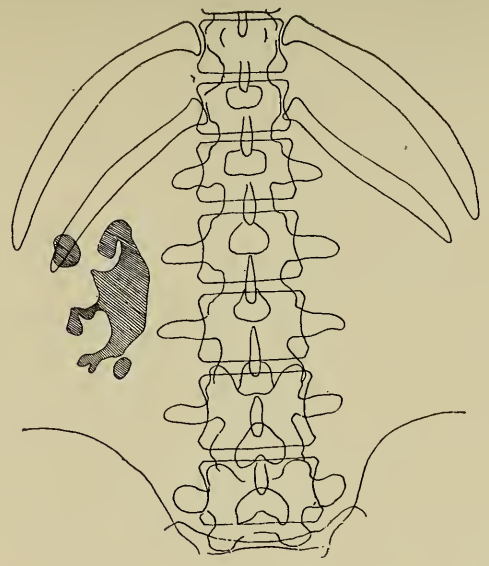

Fig. 13.—Tracing of skiagram of coral-like calculus occupying almost entire kidney. (Author's case.)

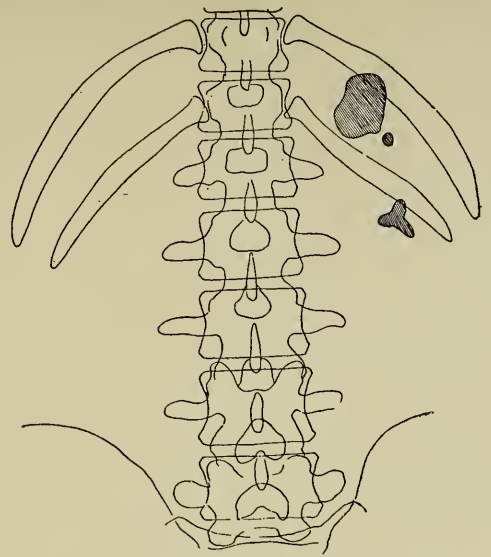

Fig. 14.—Multiple widely separated shadows lying in dilated calices of pyonephrotic kidney.

larger than those for which the operation was originally suggested. In some of my own cases we have been able to remove calculi by this route which were lying in primary calices, i. e., in the first division of the ampullary type of renal pelvis. With one finger inserted through the pyelotomy incision and the fingers of the other hand palpating the outer surface of the kidney, one is astounded at the ease with which every portion of the renal pelvis, and even the calices and papillæ emptying into them, can be felt and calculi localized. Such a bimanual palpation of the interior of the kidney has of course as its first requirement that the kidney can be brought well out into the parietal incision. My chief reason for emphasizing the intimate relations between the pelvic wall and the peripelvic vessels is to warn against any rough manipulations within the renal pelvis.

Rôle Played by X-Ray.—Granted that the patient has been thoroughly prepared and the plates are examined by one accustomed to interpret them, the present development of *x*-ray technic enables one to determine with more than a fair degree of accuracy the position of shadows with reference to whether they are in the renal pelvis or parenchyma. A shadow located on a level with and close to the transverse process of either the first or second lumbar vertebra (Fig. 8) is due, as a rule, to a calculus located at the junction of the ureter with the renal pelvis (ureteropelvic junction). Such a shadow may be triangular, round, or show a projection in a downward and inward direction. Zuckerkandl has recently directed attention to the characteristic nipple-like appearance of shadows at the ureteropelvic junction. (Fig. 9.) When a shadow of this shape and location is present, one can usually say in advance that the calculus can be removed by pyelotomy. No one will attempt to say, however, that every shadow located at the posi-

tion just described is at the ureteropelvic junction. If the kidney lies lower than normal, the shadow may be due to a calculus in the upper half of the renal pelvis, or in the renal parenchyma, close to the inner or mesial border of a low-lying kidney. (Fig. 10.) On the other hand, the shadow may be due to a calculus in the lower half of the renal pelvis of a high-lying kidney. (Fig. 11.) Further experience will, of course, show more exceptions, but the interpretation of shadows located as just described (Fig. 8) will serve for the present as a working basis.

In the absence of other complications, pyelotomy can be considered as the ideal method for calculi at the ureteropelvic junction. A peculiar shadow may be the result of calculi located partly in the primary division of a bifid pelvis and extending into the ureter (Fig. 12), or when the calculus extends in a pipe-like manner for some distance into the ureter.

Shadows Not at Ureteropelvic Junction.—When the shadow is further away, i. e., outward, from a vertical line drawn through the outer ends of the transverse processes of the first or second lumbar vertebra, the calculus usually lies in a dilated calix or in the renal parenchyma. If the shadows are multiple and are close together, the stones are probably in the renal pelvis (Fig. 12) or in a dilated calix, readily reached through the pyelotomy incision. When the shadow is single and a little farther out than where the ureteropelvic junction calculi are seen, the calculus is usually free in the renal pelvis or is lodged in a primary or secondary calix, and also easily reached through the pyelotomy incision. When the shadow is large and triangular, or coral-like (Fig. 13), or there are a number of shadows distributed over a wide area (Fig. 14), one of two conditions is usually found at operation: (*a*) either the renal pelvis is greatly dilated and filled by a large calculus,

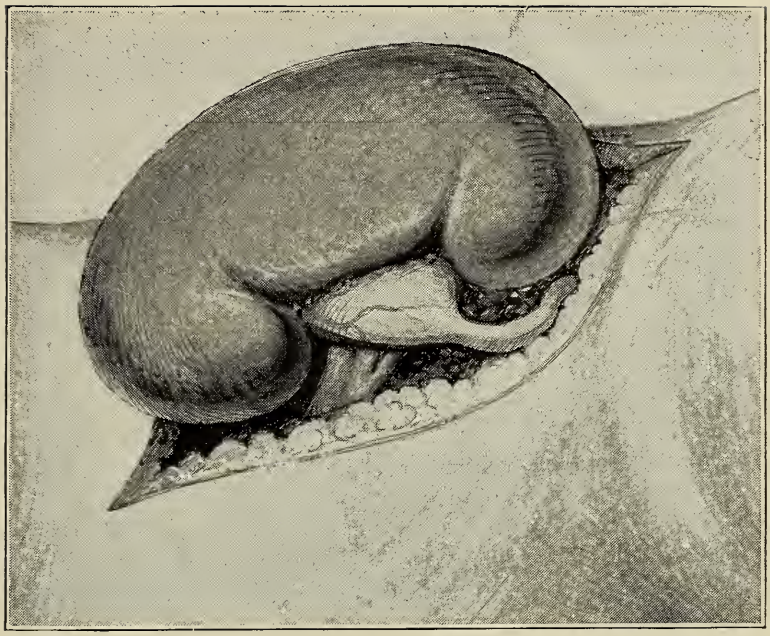

Fig. 15.—Posterior aspect of renal pelvis exposed in incision as first step of pyelotomy. Note avascular condition of the posterior aspect of renal pelvis.

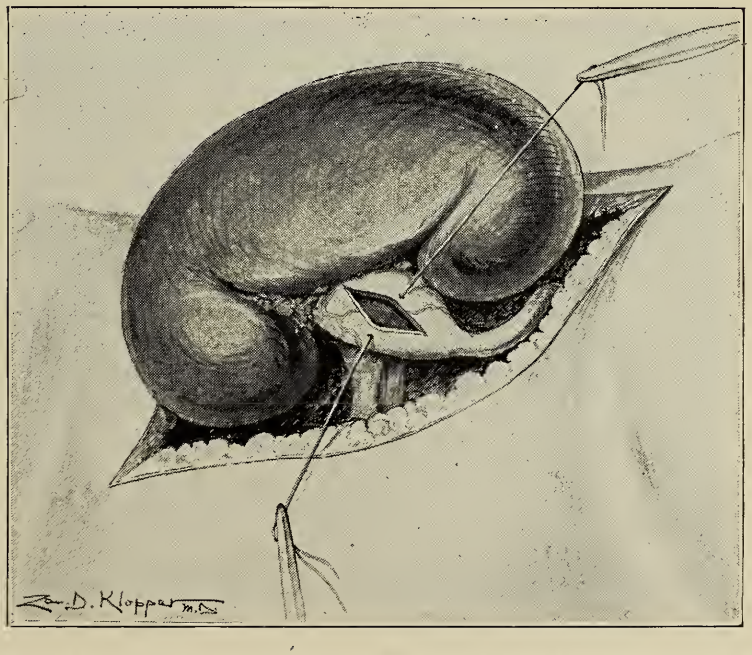

Fig. 16.—Renal pelvis opened by longitudinal incision and edges held apart by two traction sutures of catgut.

or (*b*) multiple shadows signify considerable destruction of the renal parenchyma. With such *x*-ray findings pyelotomy scarcely can be considered, although many of the French and German surgeons have become quite radical in combining incisions into the renal pelvis with those of the parenchyma in the removal of relatively large calculi located in the dilated pelvis.

It is beyond the province of the present paper to discuss the question of nephrotomy or nephrectomy when large or multiple calculi are present with extensive destruction of the kidney.

TECHNIC OF PYELOTOMY

The method of exposure and of bringing the kidney into the wound does not differ in any manner from that employed for nephrotomy. In order to perform a pyelotomy, one must be able either to lay the kidney completely on its anterior surface, on the skin edges of the incision (Fig. 15), or at least to gain access to the posterior aspect of the renal pelvis without much tension on the renal vessels. If the pedicle is short or there is much fixation of the kidney by adhesions, pyelotomy is out of the question, and too much traction under such conditions will surely result in disaster.

In the majority of cases, however, pyelotomy is possible if the incision is not too small and the wound edges are well retracted.

The method which I employ is to insert two fine traction sutures through the wall of the renal pelvis on each side of the proposed incision. The wall is then incised and the opening enlarged in an upward and downward direction with fine scissors. The edges of the incision are now held apart by the traction sutures which have been previously inserted as

shown in the diagram (Fig. 16). This procedure causes no bleeding, as a rule.

Occasionally a small vein in the fatty layer surrounding the pelvis may require transfixion. Every effort should be made to conserve the fatty tissues immediately adjacent to the renal pelvis. Calculi lying at the ureteropelvic junction can usually be felt when the kidney is turned on its face (Fig. 15), and the incision through the pelvic wall can be made directly over the calculus. Through the pyelotomy incision one can introduce the index finger and palpate the entire interior of the pelvis. The extraction of calculi lying loose in an ampulla or one of the main divisions of a bifid pelvis is very simple.

That accidents can occur when a calculus is firmly impacted in a calix is shown by a recent case of mine. The calculus was firmly held in a calix of the lower portion of an ampullary type of pelvis; it was separated by the most gentle manipulation and extracted with a blunt instrument, without any immediate bleeding. Several minutes later a steady, quite profuse amount of venous blood began to escape from the pyelotomy incision. All efforts to check the bleeding, such as tamponade and transfixion ligation of the lower portion of the renal pelvis, were of no avail and it was necessary to perform a nephrectomy. Examination of the specimen revealed that the bleeding had its origin in a small vein, just outside of the renal pelvis and in close contact with the calculus. The latter had probably caused a thinning of the wall of the vein and after the calculus was removed the pressure within the vein caused the weak intervening tissue to yield with resultant bleeding into the renal pelvis.

I have referred above to the close relation between the peripelvic veins and the pelvic mucosa. The occurrence of such severe bleeding even after the most

gentle manipulation shows the necessity of bearing in mind these vessel relations.

After removal of the calculus or calculi, the incision in the renal pelvis is closed with 00 chromic-gut sutures, which do not pass through the mucous membrane. If the urine is clear or contains only a small amount of pus, one can safely close the incision completely, insert a cigaret drain or small rubber tube down to the suture line, and replace the kidney. The danger of fistula formation is practically nil, provided that the ureteral lumen is not obstructed between the renal pelvis and bladder. It is advisable to pass a ureteral catheter down to the bladder before closing the pyelotomy incision. When the urine contains considerable pus and nephrectomy is contraindicated, a soft rubber drain is sutured into the pyelotomy incision and brought out through the posterior end of the abdominal wound.

COMPARISON OF NEPHROTOMY AND PYELOTOMY

Advantages of Nephrotomy.—1. Nephrotomy is preferable in cases in which there is a large branched calculus filling the renal pelvis, or in which there are many small calculi lying scattered throughout the kidney in closed cavities. In many of these cases, especially if the condition is a bilateral one, a finger inserted into the renal pelvis through a pyelotomy incision will aid in locating the calculi and they can be delivered through multiple small incisions through the convex surface of the kidney.

2. Nephrotomy is to be chosen in infected cases in which there is more or less destruction of the parenchyma. Here the question of whether nephrotomy or nephrectomy should be performed is to be considered, rather than any other operation. I cannot urge too strongly the necessity of ureteral catheterization of each kidney as a routine method before any opera-

tion for the removal of renal calculi. One never knows absolutely before operation whether the kidney must be removed; hence the obligation on the part of the operator of knowing the functional capacity of the opposite organ. In infected cases, when it is not necessary to remove the kidney, drainage is far better through a nephrotomy incision than through that of a pyelotomy.

3. Nephrotomy is the safer operation when the renal pedicle is short, or when there are extensive perinephritic changes which render delivery of the kidney into the wound very difficult.

4. The condition of the parenchyma can be determined much better through a nephrotomy incision. Much information as to the extent of destructive changes should have been secured, however, before operation, from the study of the clinical history, palpation, *x*-ray findings, functional tests and examination of the urine from each kidney.

Disadvantages of Nephrotomy.—1. The bleeding during the operation is far greater than in pyelotomy, even though the renal pedicle be well compressed. If the nephrotomy incision be made just large enough to admit the index-finger, while the renal vessels are compressed at the hilus, the amount of blood lost, it is true, is usually slight. If the incision be more extensive, even though it be made behind the middle of the convexity, as Zondek has shown, the bleeding may be quite alarming, especially during the removal of a large branched calculus. The employment of bisection of the kidney has fortunately become greatly restricted through the greater accuracy of our radiographic technic.

2. The danger of hemorrhage following nephrotomy is by far the greatest objection, and has been one of the chief reasons for the rapidly growing populari-

ty of pyelotomy. In addition to arteriosclerosis, hemophilia and imperfect suture of the nephrotomy incision, the most frequent causes of such postoperative bleeding are (*a*) the detachment of thrombi; (*b*) the septic erosion of vessels exposed in the line of incision; and (*c*) the bursting open of the nephrotomy wound when the ureter becomes obstructed by a blood-clot. Makkas[1] collected reports of nineteen cases, and Neuhauser[2] reports sixteen of Israel's cases. In ten of the latter, two patients died after the kidney incision had been tamponed, and in six it was necessary to remove the kidney, with three deaths, in spite of the secondary nephrectomy. Neuhauser speaks of the fact that a severe hematuria may occur after a pyelotomy, and my case, quoted above, confirms this. The danger of hemorrhage either during or after nephrotomy is much greater, however, as Haberer and others have shown, than after pyelotomy.

3. The danger of overlooking calculi, even with a good skiagram, is greater with nephrotomy than in pyelotomy. In the latter, one can palpate every portion of the pelvis and parenchyma bimanually, as previously described.

4. The danger of necrosis of the parenchyma following nephrotomy has no doubt been greatly exaggerated, but it is by no means a negligible factor in determining the method of operation.

Advantages of Pyelotomy.—1. Pyelotomy is the simplest and least serious method for the removal of calculi lodged at the junction of the ureter and renal pelvis, or for small or moderately large calculi, either free in the ampullary type of pelvis or in a primary calix of a bifid or trifid type of pelvis. Pyelotomy is also rapidly becoming the method of choice for small calculi lodged in the calices of the ampullary type

1. Makkas: Deutsch. Ztsch. f. Chir. ciii.
2. Neuhauser: Folia Urol., iv.

of pelvis. Some operators combine transverse or vertical incisions into the parenchyma with pyelotomy for the removal of larger calculi.

2. It is the best method for calculi in one or both pelves of a horseshoe kidney, as well as in bilateral cases, especially in those complicated by anuria.

3. Hemorrhage is far less likely to occur, either during or after pyelotomy, than in nephrotomy.

4. Bimanual palpation of the pelvis and parenchyma lessens the chance of overlooking a calculus.

5. Pyelotomy can be done much more rapidly, and the period of convalescence is much shorter than in nephrotomy. It is seldom more than ten to fourteen days, in my experience.

Disadvantages of Pyelotomy.—1. Pyelotomy is contra-indicated when the kidney is fixed by adhesions or the pedicle is short, or if extensive infection is present.

2. A fistula may persist after operation. The danger of such a condition is very slight, unless one has overlooked some obstruction of the ureteral lumen distal to the pyelotomy incision. A ureteral catheter should always be passed down into the bladder before suturing a pyelotomy incision.

3. The danger of bleeding, either during or after operation, from the peripelvic vessels, although slight, must always be borne in mind.

In conclusion, let me add that I believe that pyelotomy is destined to become an active competitor with nephrotomy in the majority of cases. Many of the most prominent European surgeons are enthusiastic in speaking of the many advantages of pyelotomy, and the chief object of this paper is to call the attention of American surgeons to the many merits of the operation.

Dr. William Jepson (Sioux City, Iowa): If the experience of the other members of this Association has been that of my own, then this paper is a most important one, provided that nephrotomy has been found easy by the members for the removal of calculi from the kidney.

Some nine years ago I nearly lost a patient from continuous hemorrhage. Beginning some eight or nine days after a nephrotomy, this hemorrhage ceased from time to time, but it was five or six weeks before the danger was passed and my fear allayed. This was followed a year and half afterwards by another patient, a young man, who died. I have never done nephrotomy since that time for the purpose of removing a calculus from the kidney. I immediately made up my mind to perfect myself in the technic of operating upon the pelvis or through the pelvis itself for the removal of a stone from the ureter or the kidney if accessible. Calculi in the kidney are either located within its substance and fixed, or within the pelvis of the kidney and movable. Where they are fixed, they thus oftentimes produce little harm. It is the one movable in the kidney that migrates from place to place, traumatizes, and temporarily occludes the ureter or becomes lodged in it, and gives rise to danger, since it may occlude the renal outlet at any time.

I quite agree with the essayist that the simplest thing is to open the pelvis of the kidney and remove from it whatever stones there may be found in it, and, furthermore, to palpate the calices for other stones. I will admit that I overlooked a stone in the kidney by this plan in one instance.

Dr. A. W. Abbott (Minneapolis): One of the strongest arguments in favor of the position taken by Dr. Eisendrath is the irreparable damage done to the kidney by any incision into it. If we incise we must suture. It has been shown that the injury by connective-tissue formation and fatty degeneration that follows, is in direct proportion to the work done. It is therefore inadvisable to cut the kidney to a greater extent than is absolutely necessary.

OBSERVATIONS ON THE RADICAL CURE OF INGUINAL HERNIA

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Even a cursory examination of the voluminous literature which has accumulated on the subject of inguinal hernia will seemingly convince any one that in this branch of surgery the last word has been spoken. The anatomy is well known and classically described in the current text-books. The pathology has been amply described, and the etiology is generally admitted to be based on the presence of a congenitally preformed sac. The technic has been made more uniform and is guided by certain broad underlying principles. It would seem, then, that there is no phase of the subject of hernia which has not been amply treated by the ablest of writers; yet this is not strictly the case, for there is a matter of anatomy, generally slighted, which, in my opinion, is worthy of special mention. I refer to the question of the presence, or absence, of the conjoined tendon of the internal oblique and transversalis muscles.

In the prevailing text-books on anatomy the conjoined tendon is described, but not accorded much prominence. Here and there it will be stated incidentally that the tendon may be absent. Reference to the more important works on surgery and monographs on hernia fails to elicit much information regarding the conjoined tendon. Here and there a writer will briefly mention the fact that sometimes

the conjoined tendon is wanting. Thus the impression is gained that such an anomaly as absence of the conjoined tendon may occur, but has no practical significance.

My limited clinical experience has developed the conviction that absence or maldevelopment of the conjoined tendon is not as rare as one would be led to believe by what has been written, and that the question is deserving of some notice. I am making no claims of originality, for others have arrived at the same conclusions. Thus, at the Alexian Brothers' Hospital of Chicago, where we have a fairly large hernia service, Dr. M. L. Harris and myself are finding this anomaly to be a common occurrence, especially since we have come to look for it during the last few years.

Let us first review the normal anatomy of the inguinal region. The inguinal canal is about four centimetres in length and runs from the external to the internal abdominal ring. The external ring barely admits the tip of the index finger, and it lies immediately to the outside of and above the spine of the pubes. It is formed by a splitting of the fibers of the external oblique aponeurosis into two columns or pillars. The internal ring is about midway between the anterior-superior spine of the ilium and spine of the pubes.

The inguinal canal has an anterior and a posterior wall, a roof, and a floor. The anterior wall is formed by the aponeurosis of the external oblique and by the internal oblique for about its outer third. The internal oblique and transversalis muscles arise in the male from a little more than the outer half of Poupart's ligament, sometimes even from the outer two-thirds. In the female the origin is more regularly from the outer two-thirds. In some individuals the muscles arise from the outer third of the ligament, thus afford-

ing no support to the internal ring, a point emphasized by the late Dr. Ferguson. The posterior wall of the canal is formed by the transversalis fascia and at its inner median portion by the conjoined tendon. The roof is formed by the arching fibres of the internal oblique and transversalis muscles, and the floor by the shelving portion of Poupart's ligament.

The conjoined tendon is formed by the aponeurosis of the transversalis, together with the more medial lower portion of that of the internal oblique, these two layers of fascia uniting in this region to form this so-called conjoined tendon, which is inserted into the body and superior ramus of the pubes. Medially, it is especially thickened to form a band, the falx inguinalis, which is firmly attached to the tendon of the rectus.

By closely observing the anatomy of this region during the performance of hernia operations, one will soon be convinced of the frequency with which deviations from the normal occur. The tendon is sometimes narrowed and poorly defined; more commonly it is entirely absent. In this case the fibers of the internal oblique and transversalis pass directly inwards towards the edge of the rectus without forming any tendinous union. The lower portion of the muscles as they pass inwards will be found often much thinner than normal, and there may even be spaces between some muscle bundles, exposing the transversalis fascia beneath.

Thus there is formed a triangle whose apex is at the internal ring, the sides being formed by the internal oblique and transversalis muscles and Poupart's ligament, respectively, and the base by the edge of the rectus. The floor of this triangle is formed by the transversalis fascia. This area is a very weak spot in the abdominal wall, for it is covered, and but

partially so, by the fascia of the external oblique. I am inclined to the opinion that in cases where the tendon is deficient there will be found other evidences of maldevelopment, such as an atrophic condition of the fascia of the external oblique. This structure is sometimes very thin, almost transparent, and in the region of the external ring the fibers split up and separate so as to form an abnormally large opening.

If the conjoined tendon is absent, to what extent might this predispose to direct or indirect hernia?

I fully endorse Coley's statement when he says: "At present we are beginning to appreciate the fact that the great predisposing cause in all inguinal and practically the majority of femoral hernias is a congenital or preformed sac, and on the basis of this, the first and essential principle of a radical cure must be the thorough removal of such a sac." Writers differ somewhat in their description of what they consider a predisposition to inguinal hernia. A potential hernial sac of congenital origin is always a predisposition, but cannot be detected clinically.

My conception of a predisposition, which the Germans call "weiche Leiste" or "Bruchanlage," is as follows: Clinically, the external ring will be found very large, and the canal will admit the finger for a considerable distance; there is felt a decided impulse on coughing or straining, and by sweeping the tip of the examining finger up and down with its palmar surface turned towards the median line, the absence of the conjoined tendon can readily be determined, and in its stead the blunt edge of the rectus is felt.

I believe it justifiable to operate on both sides in patients who present a fully developed hernia on one side and a "weiche Leiste," or weak groin, on the other. Where this was not done, I have had patients come back after several years with a hernia on the

Fig. 1. The inguinal regions are exposed by splitting the fascia of the external oblique muscle. On the right side is seen the usual formation and insertion of the conjoined tendon. On the left side the tendon is absent, and the muscles are inserted high up on the rectus, forming a triangular space whose floor is formed by the transversalis fascia.

other side, proving that the diagnosis "predisposition" was well founded. What is the anatomical condition which presents when we operate on these so-called cases of "predisposition?" The external oblique fascia is thin with separated fibers and a very large external ring. On slitting up the fascia and exposing the inguinal canal, the absence of conjoined tendon is verified. The fibers of the internal oblique and transversalis muscles are inserted high up on the rectus, forming a wide "inguinal interspace." The muscles themselves are likely to be weak and attenuated. In addition to this there will be occasionally found a remnant of unobliterated tunica vaginalis in the shape of a potential sac. Lastly, we may find a sac, congenital or acquired, admitting bowel or omentum, showing that the supposed predisposition was a real hernia after all.

It is freely admitted that a person presenting what would be clinically called a predisposition, can go through life without ever acquiring a hernia, just as a man may die in old age, the possessor of a congenital potential hernial sac which never developed into a hernia.

There can be no doubt that the development of a direct hernia is favored in those cases where the conjoined tendon is absent. The triangular area before mentioned, supported only by the transversalis fascia, and partly by the aponeurosis of the external oblique, is a very weak spot, presenting conditions favorable for the formation of a hernia.

Is the recognition of this anomaly of absent undeveloped conjoined tendon of any practical importance?

Our choice of operative technic should be determined by the anatomic findings. We have all seen cases in which it would have been impossible to perform a typical Bassini operation. Owing to the ab-

sence of conjoined tendon and the high insertion of the muscles into the rectus, the former could not be brought down to Poupart's ligament without creating too much tension. Either the muscle would tear at once, or necrosis would occur later. Our attention has been called to the fact, however, that this inner portion of the canal should be especially fortified against recurrence.

The technic which is best adapted to meet the requirements of these cases is the imbrication modification of Andrews. By bringing down to Poupart's ligament the edge of the fascia of the external oblique and as much of the muscle as possible, a very firm covering for this weak area is secured; this weak area is further fortified by the overlapping of the lower flap of fascia. In cases where the triangle is especially large, or the external oblique aponeurosis is especially thin, the Bloodgood technic will be found of service. By incising the edge of the rectus sheath, the fibers are exposed, and a portion of the muscle can be brought down and sutured to Poupart's ligament, thus supplanting the conjoined tendon which is absent. The operation can be completed by following the Andrews technic.

CONCLUSIONS

1. The conjoined tendon is deficient or absent more commonly than has been generally taught.

2. This anomaly supplies an important predisposing factor in the development of direct hernias, and probably plays a certain role, though a minor one, in the etiology of oblique hernias.

3. The presence of this defect precludes the performance of the Bassini operation as a routine, except in children.

4. The Andrews imbrication method best fulfills the indications, regardless of whether the tendon is

present or absent. The Bloodgood transposition of the rectus is an excellent aid when reqiured.

DISCUSSION

Dr. C. M. Nicholson (St. Louis, Mo.): I have had some experience in hernia and am surprised to hear that the cause of acquired inguinal hernia is, in many instances, due to the absence of any structure. In a large percentage of cases with which I have come in contact there has been a separation of the aponeurosis of the external oblique, the intercolumnar fibers having become so attenuated as to be easily stretched or torn, resulting in an enlargement of the external abdominal ring.

If the trouble in a large percentage of cases is due to the absence of any structure, it seems surprising that, clinically, we get such excellent results without replacing that structure.

In somewhat over five hundred herniotomies, cases in which no complication existed, I have seen but three recurrences and these were all due to a condition other than that described by the essayist. All three were in children, and all were due to a lack of development of the aponeurosis of the external oblique muscle, so that a failure of support existed, even though the thin, delicate aponeurosis had been overlapped.

In congenital inguinal hernia the gut or omentum comes into direct relation with the testicle, and we know a preformed sac exists in such cases, but I have had no evidence to convince me, that in inguinal hernia, other than the congenital variety, the theory of a preformed sac is tenable.

I believe the cause of most cases of inguinal hernia, except the congenital variety as above defined, and that variety passing directly forward through the external abdominal ring, is the lack of support usually afforded by the aponeurosis of the external oblique.

Dr. A. E. Benjamin (Minneapolis, Minn.): I think the essayist is right in his conclusion that the conjoined tendon is very often absent. I have found a great many cases where this was the fact. It has been my habit in every case to transplant the sac. After tying it off, I pass it back of the transversalis and bring it up high so that the intra-abdominal pressure in the sac afterwards comes against the transversalis muscle. In that way we take off future strain. The fibers of the transversalis and internal oblique are then brought underneath Poupart's ligament with a modified Halsted mattress silkworm-gut suture, bringing the suture out through the skin. This was done be-

cause when I first developed the operation we did not have suitable catgut or reliable suture material which we could trust. At the present time this does not obtain. I have found it is an admirable operation in nearly every instance, because we find tying these sutures over bits of gauze takes off the strain. There is no cutting of tissue with sutures at all; we have a broad approximation of the flap; and we have the transversalis fitting at all points firmly against Poupart's ligament; but with the other method in suturing by continuous or interrupted suture, there is more or less separation of the fibers and weakening of the wall. In this way we can make more tension on the transversalis and internal oblique and bring them underneath Poupart's ligament. I supplement this by overlapping Poupart's ligament and the external oblique with the cord outside.

Dr. F. Gregory Connell (Oshkosh, Wis.): I agree with those who have spoken as to the necessity of operating on the other side where there is a slight bulging, even though there may be no demonstrable hernia, as I have had the same experience of subsequent hernia developing on the sound side following the repair of one side.

Absence or obliteration of the conjoined tendon I have met with. In such cases we may have a direct hernia, the bulging being below the deep epigastric vessels, being in reality a ventral and not an inguinal hernia, differing from the usual ventral hernia only in presenting through the external inguinal ring. Or because of this weakness of the conjoined tendon in the oblique inguinal hernia, the internal ring will be dilated by the presence of the sac and its contents until the epigastric vessels are forced down almost to the pubes, so that, anatomically, it is still an indirect or oblique hernia, but practically it is a direct hernia.

By repairing such herniæ in the manner suggested by Dr. Hessert, the internal oblique muscle being brought under the cord one does not get an anatomic result. The new canal is then straight through the abdominal wall to the aponeurosis of the external oblique, and there is no external active muscular buffer to push back any viscera that might project through the new internal ring that has been formed.

By uniting the rectus muscle and fascia to the under shelving of Poupart's ligament a new internal ring may be formed that will be in its normal position and relationship so that the canal will have its normal length and obliquity. By next bringing the internal oblique to Poupart's ligament, the new internal ring will be protected, as it is in the normal. Closure of the divided

aponeurosis of the external oblique will complete the anterior wall of the canal and the external ring. The result is now entirely anatomical. Taking each structure, it will be found that they are in the relationship to each other that will be found in the normal.

Dr. M. L. Harris (Chicago): As the essayist has said, for some years past at the Alexian Brothers Hospital, we have made careful notes of the anatomic conditions in our operations on hernia. As the result of these observations I am thoroughly convinced personally that all hernias that develop during the active period of life are congenital in origin. By this I mean they are due to congenital defects. I will exclude the hernias that develop in the aged, coming at the period of life when there is general atrophy and wasting of the tissues in old people, with chronic bronchitis, emphysema, enlarged prostate, etc. Every hernia developing in the inguinal region during the active period of life has its origin in a congenital defect. The indirect herniæ with typical hernial sacs, I believe, all occur in congenital sacs.

I have had the opportunity of operating on several so-called traumatic herniæ, herniæ that appear soon after an injury, as a fall, etc. In every one of these cases I have found unmistakable evidences that the sac has been congenital, and there is never any difficulty in determining at the operations if the sac is of congenital origin.

That the absence of the conjoined tendon is a material factor in the causation of herniæ has been shown by demonstration by an article which appeared a couple of years ago from Russia. It was discovered that during the last Russian war an unusually large number of conscripts were constantly appearing with ruptures, otherwise healthy young men, who were capable of going into service, but escaped service by reason of having ruptures. The authorities investigated the matter, and after getting confessions from some of the subjects, it was discovered there was a regular place where these young men went and had these hernias produced, and they were produced by a young doctor who had discovered that by passing his finger up into the external ring and forcibly tearing the conjoined tendon from its insertion, that these young men would all develop hernias within a short time. In this way he produced hernias in these individuals. He never touched the internal ring. He never passed his finger to enlarge the internal ring, but put the finger in and pulled upward and inward to tear the fibers of the conjoined tendon. If he could tear these fibers, then these indi-

viduals would all develop herniæ. That demonstrated absolutely the influence of the conjoined tendon in producing hernia. Its absence we have found to be a common occurrence.

A congenital defect in the region of the origin of the internal oblique from Poupart's ligament is also another factor of importance in the development of indirect hernia, and it is also an important factor in recurrent hernia. I have had the pleasure within the past ten days of operating upon one of my own cases that had a recurrence. I operated on the man about a year ago or more. He returned recently with a beautiful recurrent hernia on one side. At the operation I found that the hernia had come down entirely outside of the internal ring, entirely outside of my line of suture, and at a point where there was absence of the internal oblique, or, rather, a short origin of the internal oblique from Poupart's ligament, entirely outside of the cord, leaving a weak triangular space, and through this space a hernia had developed. This is not at all new, because we have had our attention called to that space a number of times. When I find it, I close it at the original operation.

Another reason for recurrence is a defect in the external oblique beyond the point where we usually stitch it up during our operations. This may be a defect in its thickness or a defect by reason of the absence of the intercolumnar fibers, allowing the external oblique fascia at this point to split.

From these observations and studies, I feel that every hernia that develops during the active period of life, and is not produced directly by some such method as I have mentioned or by a direct injury, which has the same effect, namely, a tearing of the fibers of the conjoined tendon or the muscle, is of congenital origin.

Dr. Daniel Eisendrath (Chicago): Fortunately, in the hospital where we are located in Chicago, we have a large number of herniæ, and there are a great many valuable points Dr. Hessert has brought out that I think cannot be too strongly emphasized. In the first place, our observation of this class of cases has taught that it is possible to make a diagnosis before operation. We make it a rule and ask the patient to let us operate on both sides, even though on the other side no hernia has appeared, because, as Dr. Hessert has said, sooner or later a hernia will appear on the other side, and the patient may feel that there has been some negligence on our part in having operated on one side only. He cannot associate that with the fact that he has continually weak muscles there.

One point Dr. Hessert spoke of was this: He said, in addi-

tion to the fat, oftentimes there is imperfect development or lack of development of the conjoined tendon, or atrophy of the internal oblique arching fibers. This condition is found when the conjoined tendon is fairly well developed. We find it in stout individuals who have attenuated oblique muscles, where there is practically only white muscle, and lying between the white fibers we find quite a lot of fat. What good does it do to perform the old Bassini operation? You invite disaster. If you suture this attenuated internal oblique, weak or imperfect conjoined tendon to Poupart's ligament and put the cord on top of it, you will have trouble or a recurrence, no matter whether you transplant the cord or not. Those who are acquainted with the operation have adopted almost as a routine the plan of Dr. Andrews of transplantation or imbrication of the muscles, because it has a tremendous advantage. If you have such a conjoined tendon absent or weak or poorly developed internal oblique, it passes behind the cord, and there is a strong layer of aponeurotic structure which simply cannot be excelled. It is thin, and it will take a whole lot of tendon to put that together. The principle of the operation is to go to work and suture, beginning down here with Gimbernat's ligament and suturing with kangaroo tendon. It lasts longer, is much less liable to suppurate, and does less damage with a relatively small-sized caliber than any other form of suture. You put in a continuous suture of kangaroo tendon up to the lower border of the cord. I stop at that point where the cord comes through, and that bears out what Dr. Connell brought out, namely, you have not as good protection. I make use of Coley's modification of the Bassini method. Introducing the finger here alongside the cord so as not to make the opening too small, I put in another kangaroo suture above the cord, and in that way we can limit absolutely the internal ring opening. I leave the other suture alone then and go on back again with the first suture, go to work and bring the internal portion of the external oblique over to Poupart's ligament in the same way, over on the top of the other, making a continuous suture in the same way, then close this up above the cord, laying the cord on top of it, and making a double-breasted operation. On cross-section of such a hernia operation you see the cord here in the center. Then, you have this (illustrating) if this should represent Poupart's ligament. We have here the internal portion of the external oblique and the internal oblique lying behind the cord, and the external portion of the external oblique on top of the cord. In transplanting the cord up here (indicating), if you do transplant it, you can regulate the size of the opening

by means of sutures. You have muscular tissue to deal with. If you do not transplant the cord, you have pubic bone on one side and muscle on the other side, and in this way you are apt to get recurrences.

Dr. Hessert (closing the discussion): The first gentleman who discussed my paper evidently did not understand my statement. I make no claim whatever that the absence of the conjoined tendon is the cause of hernia, except as having a certain bearing in the formation of direct inguinal herniæ. I think Dr. Harris put it tersely when he said that herniæ which occur during young adult life are based on a certain developmental defect. It is either a congenital hernial sac or some developmental defect and absence of the conjoined tendon or peritoneal muscles or fascia in this region. Added to that, you must have predisposing factors which bring down the hernia.

I had an experience not long ago in New York in attending one of the clinics held by a prominent New York surgeon. He was doing a typical Bassini operation, and telling his audience how to suture the conjoined tendon to Poupart's ligament. I ventured to ask him what he would do in case the tendon was absent. He regarded me with a look of suprise and said he always sutured it. That ended the discussion. If a man of his experience does not pay any attention to the absence of the conjoined tendon, I thought it was of sufficient importance to be mentioned, and so I made it the subject of a little paper. The object of the paper really is to bring out the fact that the tendon is absent frequently, and in cases of this kind the typical Bassini operation is not the proper one to perform, and any one who does a Bassini operation as a routine measure will have recurrences if he attempts to bring the internal oblique and transversalis from their high insertion on the rectus down to Poupart's ligament, as advised in the typical Bassini.

ACCIDENTS AND COMPLICATIONS OF HERNIA, WITH REPORT OF SOME INTERESTING CASES

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The most common accidents and complications of hernia are inflammation, incarceration, strangulation, obstruction of the bowels, and gangrene.

Owing to the peculiar anatomic conditions permitting hernias, traumatic inflammations due to a devitalization of the tissues, are liable to occur. The tissues may be infiltrated, and thickening of the hernial sac may be produced by the long-continued use of a truss. The fascia from stretching may become very attenuated.

In some patients with fat, pendulous abdomens, very marked thinning of the fascia with diastasis of the recti muscles may occur.

Kelly says every grade of inflammation is possible. At times the process is so mild and chronic that the hernial contents become adherent without special symptoms, some hernias undergoing spontaneous cure through the process of chronic inflammation.

At times the inflammation occurs in mild acute attacks in which the patient suffers pain and tenderness in the hernia region. Such attacks may follow excessive or unusual exertion which produces circulatory changes within the sac or causes increased irritation by the truss, or they follow external violence.

Irreducibility.—Mild acute attacks of inflammation of a hernia generally subside without special treatment and with no appreciable gross change in the rupture, but permanent adhesions may form and result in irreducibility. The volume of the contents of the sac may become too great to permit its return through the neck of the sac. The omentum is the organ that most frequently becomes irreducible. Sometimes for years a section of omentum may occupy a hernial sac and be perfectly reducible, when a slight inflammatory change causes the lumen of the sac to become so small that the omentum cannot be replaced. Any pressure on the hernial ring tends to shut off the venous flow and cause acute or chronic passive congestion, thus augmenting the hernial contents.

The natural increase in size of the omentum incident to advanced life often interferes with its reduction. (Kelly.) Tumors of the mesentery have caused the same result. In children hernias rarely become irreducible. Femoral hernias are more difficult of reduction than inguinal. Frequently the conditions of inflammation and irreducibility are present interacting and resulting in strangulation.

Strangulation is produced when the compression in the hernial ring or at the mouth of the sac is such as to entrap the contents of the hernia and cause blood-stasis. The strangulation is often temporary and by proper measures may be quickly relieved, but unheeded the tendency is to become more serious and may result fatally. On the contents of the hernia in a measure depend the rapidity and seriousness of the process; but no definite rule can be formulated, for an intestinal strangulation may at times extend over a period of several days without serious injury to the bowel, while an omental strangulation may rapidly lead to infarct, necrosis, infection, and death.

The mechanism of strangulation may be either an

elastic constriction or a sudden increase of the fecal contents of the herniated loop of intestine. The unusual pressure at the neck of the sac may cause complete stasis of the circulation and anemic infarct of the hernial contents. Venous stasis is, however, usual at first. Transudation of serum follows, necrosis of the tissues may result, and if the contents be intestine, perforation may be produced, resulting in a fecal fistula. Septic peritonitis and death may result from extension of the inflammation to the peritoneal cavity.

Symptoms of Intestinal Strangulation.—Pain, sudden in its onset with some preceding unusual violent muscular strain, is the most common symptom. Swelling follows, and the patient is unable to reduce the hernia. Nausea, eructation, and vomiting soon supervene. Fecal vomiting may occur if the obstruction is low in the intestinal tract. No flatus or feces are passed per rectum, although of course enemas may wash out fecal matter below the obstruction.

Manipulation of the hernia may cause severe pain, owing to the tense edematous swelling or the complicating inflammation. At times there is nausea without vomiting; considerable quantities of mucus and feces may be expelled when there is local inflammation of the hernia.

The pulse grows small and its rate increases; the temperature may rise or it may be subnormal. The secretion of urine may be diminished, and complete suppression may be the cause of death.

Strangulation, according to Berger, is about twice as frequent in the female as in the male. In children, strangulation is rare and treatment by taxis is usually successful.

Occasionally in strangulated hernia only a portion of an intestinal segment becomes involved. In such cases the symptoms may be severe pain with nausea and vomiting, but the intestine, not being completely

obstructed, may respond to cathartics and enemata.

This class of hernia occurs most frequently (Kelly) in the femoral and obturator regions.

Tuberculosis of the hernia sac is rare.

Carcinoma of the hernial contents has been described.

Hemorrhage from the bowels may follow the reduction of a strangulated hernia.

Intestinal obstructions, due to adynamic ileus following an operation for strangulated hernia, may occur. With the modern method of post-operative treatment, e. g., the use of the Fowler position and enteroclysis, with the drop method of physiological salt solution, this complication is prevented or its frequency minimized.

Inflammation of the omentum sometimes follows operation for strangulated hernia. This may cause a hard abdominal tumor, which slowly disappears, or it may suppurate.

Infected thrombi from the mesentery of the gut may be carried to the lungs and cause septic pneumonia.

The transverse incision for umbilical hernia as devised by the Mayos by lessening the tension in the wound, diminishes the chances of post-operative pneumonia, as the patient can breathe more deeply without pain, thus expanding the air-cells of the lungs. The importance of this has been emphasized by the late von Mikulicz and J. B. Murphy.

Crossen states that such structures as the ovary, Fallopian tube, uterus, and even the pregnant uterus have been found in an inguinal hernia.

Pregnancy complicated with vaginal hernia requires careful handling.

Dr. M. R. Stewart of our staff recently operated on two cases of right inguinal hernia complicated with unilateral cryptorchidism.

Another of his cases of strangulated inguinal hernia necessitated the resection of a foot of intestine. He used a Murphy button for anastomosis of the bowel. Recovery was uneventful, the button being passed in seven days.

That a portion of the urinary bladder sometimes is included in the contents of an inguinal hernia is a well-known fact. Dr. Kiliani states (*Annals of Surgery,* vol. 36) that this complication exists in 1 to 2.5 per cent of cases. Becker of Braun's clinic met with it in 30 per cent of cases of very large hernia.

In one of the surgical clinics of Chicago recently a patient was operated on for inguinal hernia. The bladder was unintentionally injured and not being recognized was not sutured. Peritonitis resulted, and the case terminated fatally.

A gangrenous appendix is sometimes found in a right inguinal hernia. Such a case occurred in the surgical service of Dr. Union Worthington.

Recurrence after operation depends on various factors, e. g., incomplete removal of the sac, the method of fixing the stump of the sac, but, most of all, sepsis in the wound.

Bloodgood shows that in Halsted's clinic there were but 3 per cent of returns in hernia cases healing by primary intention, while in suppurating cases there were 20 per cent of recurrences.

REPORT OF CASES

Case 1.—Mrs. H.; age 54; American, widow; mother of nine children; very fleshy; weight about 200 lbs.

Family history: Father died at 60 years of age from cancer of the stomach; mother died at 40 from effect of post-partum hemorrhage; three brothers living; one sister died of puerperal sepsis at 29 years of age; another died of some pelvic trouble at 30 years of age.

Personal history: Health good up to time of marriage at 17 years of age; mother of nine children; last baby still-born.

Present trouble: At the birth of her first child she noticed

a small swelling in the region of the umbilicus. This gradually increased in size until it became as large as a new-born child's head. She had consulted several physicians at various times, but, although she suffered severely at intervals when the hernia became partially incarcerated, she refused operation for several years. On several occasions I replaced the hernia. When seen (Aug., 1907) the surface of the skin over the hernia was ulcerated and infected. The tumor-like hernia could not be replaced in the abdomen. She consented to operation, which was performed (August, 1907), Mayo's technic for umbilical hernia being used.

A ten-inch transverse elliptical incision was made.

Result: There was some breaking down of the fatty tissue. Good union, however, later occurred and complete cure. Patient now is in excellent health and is able to do a hard day's work. The scar is contracted to six inches.

Case 2.—Strangulated umbilical hernia. January 10, 1912.

Mrs. P.; aged 58; nativity, Swedish; occupation, housewife; very stout, short, and fleshy; weight, 180 lbs.

Family history: Negative.

Personal history: Health good up to time of marriage; married at twenty-seven; mother of five children. During her fourth pregnancy at about the seventh month she suffered from a fall, and an umbilical hernia developed. It was at first about the size of a walnut. During the last two years it became enlarged until it became three and one-half inches in diameter. During the last year she suffered at intervals with severe pains in the region of the hernia.

When I saw her about noon of January 10, 1912, she had been suffering three days; she had an anxious expression, severe abdominal pain, distention of the bowels, and eructated gas having a fecal odor from the stomach. I advised immediate operation. She was taken to Judge Mercy Hospital and operated on at 4 p. m. January 10, 1912.

A transverse elliptical incision was made about eight inches long surrounding the umbilical hernia, a mass of omentum and fat weighing about two pounds being removed. The operation was completed according to Mayo's technic—a loop of small intestines about four inches long was compressed and partially strangulated. As the bowel responded slightly to the application of normal salt solution a resection of the gut was not considered necessary.

Result: Patient was severely ill for 48 hours, suffering con-

siderable shock, but after that rapidly recovered and left the hospital in two weeks—entirely well.

Case 3.—Empyema of right femoral hernia sac.

Mrs. K., a woman about 38 years of age, referred by Dr. Noonan.

Symptoms: Pain and tenderness in the right Scarpa's triangle. Gave a history of right femoral hernia.

Operation January 9, 1912. Removal of sac; anchoring of stump of sac and drainage.

Result: Recovery.

Case 4.—Man, aged 74; married; ocupation, private secretary.

Family history: Father lived to 76 years; mother died at 90 years; two brothers and four sisters; with the exception of two, all lived to old age.

Personal history: During the last year of the Civil War a horse fell on him and crushed him, resulting in bilateral inguinal hernia; he wore a truss for about thirty years. At times the hernias became excessively large and difficult to replace. The left rupture was about the size of a large grape-fruit; the right hernia was somewhat smaller and was complicated for about twenty years with a hydrocele of the tunca vaginalis testis.

During the last year (previous to the operation) he had retention of urine and on several occasions complete urinary obstruction due to a complicating enlargement of the prostate gland. For a number of years he refused operation, believing his condition too severe to justify surgical intervention, until finally on account of the complete urinary obstruction it became imperative in order to relieve his extreme suffering and save his life.

Operation: Briefly his operation was as follows: (Associated in operation were Dr. H. S. Scott and Dr. Tyree). 1 External perineal urethrotomy to afford temporary relief from the complete urinary obstruction. 2 A few days later we did a bilateral herniotomy (Halsted's technic) under spinal anesthesia and tropococain. 3 Complete perineal prostatectomy (Young's technic modified). Spinal anesthesia with tropococain.

Result: Complete cure of both hernias and relief of urinary obstruction.

Later history: About a year later (December 4, 1912) patient was stricken with left hemiplegia due, in my opinion, to a

hemorrhage in the brain induced by arteriosclerosis and high blood-pressure. The paralysis has so far improved that the patient is able to walk around and is fairly comfortable.

Case 5.—Mrs. M.; aged 43, American; native of Minnesota; height, 5 feet 4 in.; weight, 185 lbs.

Family history: Father living, aged 70 and in good health; mother died at 39 years, of paralysis; five brothers and three sisters, all well.

Personal history: Health good up to time of birth of first child at 17 years of age (ten months after marriage); forceps delivery.

Two and a half years later perineum was badly torn at birth of second child. Twelve years ago was operated on in Great Falls, Mont. Perineum repaired and cystic tumor of left ovary removed, and round ligaments shortened. This operation was followed a month later by a ventral hernia. There had been no suppuration, but the patient had suffered much from nausea and vomiting. About two years later an umbilical hernia developed. The umbilical hernia ring was about three-quarters of an inch in diameter. About six years ago both the hernias were operated on at Rochester, Minn., where the patient was in the hospital twenty-seven days. Wound healed by primary union. About four months later the hernias recurred, although the patient had worn a supporter.

About five years ago I first saw the patient. She was then suffering from a ventral and an umbilical hernia. She suffered severely at times from inability to replace the ventral hernia.

Two years ago she was admitted to Judge Mercy Hospital where I operated on her again. At this time there were three hernias, one umbilical about one inch below a small ventral hernia, and a larger ventral hernia in the lower angle of the previous operation. The umbilical was operated on by a transverse elliptical incision, and the ventral by a vertical incision. There was some necrosis of the fatty tissue following the operation. Knocker's method of treating the sac was used in one of the hernias. There was considerable difficulty in relieving the tension in the lower portion of the vertical incision, owing to the excessive amount of scar-tissues and the great amount of intra-abdominal pressure due to fatty tissues.

Result: Relief from the partially incarcerated condition of the ventral hernia and cure of the umbilical and upper ventral hernias. The patient wears a corset to support the lower part of

the abdomen and the weakened abdominal wall, and is relieved from her abdominal pain.

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SPINAL ANESTHESIA

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MANKATO, MINNESOTA

If one were inclined to be a plagiarist in the preparation of a paper to present to this Association he certainly would not select spinal anesthesia for his subject. After writing this paper, for the purpose of checking up myself I looked through the numbers of the *Journal of the A. M. A.,* back as far as 1903, and could find almost nothing bearing on this subject.

It is perhaps more strictly correct, and in accordance with the rules of philology, to entitle my paper "Spinal Analgesia," but I have seen fit to use the more conventional form, "Spinal Anesthesia."

The name *anesthesia* as applied to artificial loss or deprivation of sensation, was first suggested and given to the profession by the late Oliver Wendell Holmes.

The best anesthetic to use has been a mooted question since William T. G. Morton, M. D., of Boston, gave ether to a patient for tooth-extraction on September 30, 1846, and a little more than one year later, November 15, 1847, Sir James Simpson successfully administered chloroform at the Royal Infirmary. The discussions at first were as to the relative merits of nitrous oxide and sulphuric ether. Later these anesthetics (nitrous oxide and sulphuric ether) and chloroform, all had their advocates, some preferring one and some the other; some claiming that chloroform was the best, and others claiming that sulphuric ether was the best, nitrous oxide occupying a subsidiary

place. Since the above dates many substitutes have been used by the profession, but their life has been short and unsatisfactory, except nitrous oxide and oxygen, and this mixture used as an anesthetic is in the experimental stage. After a trial of more than one-half of a century, sulphuric ether stands preëminent as a general anesthetic. Many local anesthetics have been discovered, but, like most of the general anesthetics, have been relegated to past history. Cocaine and ethyl chloride alone having stood the real tests. Of these cocaine has by far the preference. The effect of ethyl chloride is purely surface and very transitory. Cocaine alone survives as the most reliable local anesthetic. It is the alkaloid of cocoa leaves and combines with various acids in the formation of salts. As to local anesthesia cocaine did not come into very general use until after the experiments of Karl Koller, who reported these experiments to the Congress of German Oculists at Heidelberg, in 1884.

Experience has taught the profession one valuable lesson in the use of cocaine since its introduction, that is, that a much weaker solution will give a more excellent local anesthesia than was first thought possible. Using it hypodermically we have dropped from a 4 per cent solution to a one-half of 1 per cent, or even less; and thus the toxic effects are obviated. This is the most valuable and reliable drug used for spinal anesthesia, and is safe in the above diluted strength, it having been found preferable to eukane or stovain. So delicate is the spinal cord, and so easily injured by traumatism, and so serious is it to introduce sepsis into the spinal fluid, that many surgeons shrink from introducing a needle through the membranes into the spinal fluid, lest by accident the needle goes a little too far and injures the cord. Of course, the latter danger is largely obviated by selecting a point low down in the lumbar region, for the

cord, *per se,* does not extend below the border of the first lumbar. This fear is well founded, especially provided the lumbar puncture is not used, yet, with care, the accident can be avoided. The lumbar region is therefore the place of election. A good needle with a short bevel should be used, and it must fit closely on the syringe, which has a shoulder rather than a thread. The reason for this is apparent, for if the bevel of the needle be long there is greater probability of injuring terminals of the cord with the point. When the needle is once introduced the point should be held steadily. This cannot be done if a needle with a thread attachment is used. The syringe should have a shoulder and not a thread.

The solution of cocaine must be thoroughly sterile. Boiling destroys its virtue; therefore it may be prepared as follows: Take five grains of cocaine and mix in a thoroughly sterile mortar with one drachm of sulphuric ether rubbed until the ether all disappears by evaporation, and the powder is left dry; now add one or two ounces of sterile normal salt solution according to strength desired. One ounce gives a solution of 1 per cent and two ounces of one-half of 1 per cent, approximately; the later is sufficiently strong. One c.c. of this solution is a sufficient dose. The skin should be made as sterile as any field of operation, for it will not take much sepsis to induce a meningitis, or possibly a cerebrospinal meningitis. It is best to scrub the skin well with green soap two or three hours before the operation, then cover with a dry antiseptic dressing and leave this until one is ready to make the puncture, then apply tincture of iodine in the usual way. The point selected for puncture may be, and perhaps should be, between the third and fourth lumbar vertebræ, and about one-half inch from the median line. The fourth lumbar spine is on a line with the crest of the ilium. The skin

may and should be anesthetized with ethyl chloride, or by the injection of a little of the above prepared cocaine solution into the skin. It is better before inserting the needle to make a small incision in the skin with a sharp small knife. The anesthetist, if I may so designate him, must have his hands as completely sterilized as are his instruments, or, better still, wear sterile rubber gloves. Pass the needle upward and inward until it reaches the cavity, then remove the syringe and allow about 1 c.c. of the spinal fluid to escape, or a quantity equal to the cocaine solution injected. The syringe, now loaded with about 1 c.c. of the cocaine solution, is refitted to the needle, and the quantity slowly injected into the cavity. Anesthesia is almost instantaneous.

I have described the technic of spinal anesthesia somewhat minutely, even at the risk of being tedious. I have found that comparatively few physicians have ever used spinal anesthesia.

The line of anesthesia will not rise much higher on the body than the point of puncture, but a higher anesthesia can be obtained by lowering the body of the patient. This must be done with care.

The condition of the anesthetized tissues under spinal anesthesia differs in some respects from that which obtains under chloroform or ether. The patient can feel pressure or pulling. He feels nothing when an incision is made with a sharp knife, but will recognize the pulling or pressing of a dull one. The steps of an operation under spinal anesthesia are, of course, the same as under chloroform or ether, as, for example, in inguinal hernia, but every step must be taken with great care and deliberation, or pain will be inflicted. Retractors must be carefully applied and gently pulled upon. The operator can lift up the tissue with tissue-forceps, but he must not pull.

When I see some surgeons operate that are otherwise good surgeons, but handle the tissues of a patient under chloroform or ether as they would handle them in making a post-mortem, I think that it would be a good thing for them to operate a few times under spinal anesthesia, that they might have more regard for living tissue. Pulling and tearing and unduly pressing living tissue is always bad surgery.

One cannot operate as rapidly under spinal anesthesia as under chloroform or ether, as may be inferred from the above. For this reason busy operators like the Mayos, at Rochester, Ochsner, of Augustina Hospital, and Murphy, of Mercy Hospital, cannot afford to use this form of anesthesia unless its superiority is demonstrated over chloroform and ether.

I have had no experience with eucaine, but have seen it used several times, and my observation is that it is inferior to cocaine, because it produces a "patchy" anesthesia. I cannot explain why this is. Under spinal anesthesia the patient is perfectly conscious, and can watch his own operation. There seldom are nausea and vomiting. I have had no experience with spinal anesthesia in obstetric work, but theoretically it would be ideal.

What are the dangers of spinal anesthesia? Practically none, provided the cord is not injured, or its terminal branches, and no sepsis introduced into the spinal fluid. I have no reliable statistics as to deaths from spinal anesthesia, but I think a very small number have died from shock and heart paralysis. Bier, who was a pioneer in spinal anesthesia, states that in his last 339 cases vomiting occurred in only 5 per cent, and pallor, nausea, and brief disturbance of the pulse in only 9 per cent. Headache was noted in 6 per cent, but exclusively in women who were subject

to headache. Spinal anesthesia is regarded as indicated only when local anesthesia is inadequate and general anesthesia is contra-indicated.

What are the advantages of spinal anesthesia over chloroform and ether? Very few. The judicious administration of sulphuric ether in the hands of one who is qualified to administer it is attended with very little danger, and the operator has the patient fully under his control. True, nausea and vomiting are likely to follow ether, but this results only in a little unpleasantness to the patient. There is no disadvantage in the patient being unconscious when he is being operated upon, and I believe it is an advantage. In reference to the danger of sulphuric ether: Primarily, it is almost nil, but I think there are more cases of ether pneumonia and acute nephritis than most physicians recognize, although nephritis is usually transitory. Under the hand of a careful, skilled operator the tissues of the patient will not be abused any more under chloroform or ether than under spinal anesthesia. Under the hand of the careless, unskilled operator the tissue cannot be abused under spinal anesthesia, but it is better for the latter to go out of business.

The danger to the patient and the unpleasant effects of ether anesthesia are not increased in proportion to the time the patient is under its influence, as was formerly taught, provided that time is reasonable, e. g., all other things being equal, a patient under ether for two hours is in no greater danger, or the after-effects are no worse, than if the same patient were under ether one hour. Except in very exceptional cases, unless the operator can complete the operation in from one to two hours, he ought not to operate at all. Therefore, I believe that spinal anesthesia has come to stay, as applied to cases where a general anesthesia is contra-indicated, yet, it will never supplant chloro-

form and ether in general surgical practice. Dr. Bainbridge of New York says "the indications for spinal analgesia are the contra-indications for inhalation anesthesia." If this be true its usefulness is very limited.

DISCUSSION

Dr. James E. Moore (Minneapolis, Minn.): I agree with the essayist that spinal anesthesia is indicated only when general anesthesia is contra-indicated. I am disposed to differ with him, however, in the matter of danger of local anesthesia, and the principal objection to spinal anesthesia is its danger. The most recent case, and rather dramatic, was the instance at the meeting of the Clinical Congress of Surgeons in New York City, where a patient lost his life on the table from the administration of spinal anesthesia.

Spinal anesthesia was first suggested by Corning, but first put into practice by Bier in 1898, who afterwards abandoned it for a time on account of the mortality. In 1903 and 1904 stovain and novocain were introduced. At the present time novocain is the favorite drug for this purpose. In the University Hospital of Minnesota we use no other drug for local or spinal anesthesia, and so far we have had no mortality and no uncomfortable symptoms. We have used it locally for hernia operations and the like. We have used venous anesthesia in the University Hospital, injecting one-half of one per cent solution into the veins below the constrictor. In a case in which both legs were removed, the patient was sitting up thirty minutes thereafter eating dinner, something that could not be done after any other form of anesthesia. In the use of novocain, anesthesia is prompt and continues from half an hour to an hour and a half. The danger is reduced to a minimum by the drug, but there is still more danger than from a general anesthesia. Thus far spinal anesthesia has been used mostly by experts. We can readily understand that if it were to become more popular and were to be used by the unskilled, the mortality would increase. British surgeons, who have given spinal anesthesia extensive trial, absolutely condemn it as a routine method of anesthesia. They claim it should be limited to regions below the diaphragm or, better, below the umbilicus; that it should not be given to a patient under seventeen years of age nor in the Trendelenburg position; that it should not be given to nervous patients, because they are conscious and they are too much disturbed. It should not be given

when disease of the cord is present nor when there is a local septic condition for fear of conveying septic material into the spinal canal. It may be indicated at times in pneumonia, in tuberculosis of the lungs, pericarditis, pyelitis, renal conditions, threatening coma, and diabetes.

I had occasion to learn that spinal anesthesia is not very popular in Minneapolis, because this last summer I wished to perform an amputation in a case of diabetic gangrene, and I did not want to rely on a general anesthetic. I tried to get nitrous-oxide anesthesia, but could not, and finally I tried to get gas and oxygen, and I could not get them. I can get it now, but last summer I could not. I could find no one who was interested in spinal anesthesia. The one man who had given it was absent. The patient had ether as an anesthetic and died a few days after the operation. He might have survived had I given spinal anesthesia or even gas and oxygen. At times, when a patient is *in extremis* from intestinal obstruction, spinal anesthesia may be advisable, and the same is true with strangulated hernia. It has been used more in Minneapolis for the removal of the prostate than any other one agent. It is suggested by English surgeons that it might be used with advantage in cases of ectopic gestation when the patient is in collapse.

Dr. M. L. Harris: I wish to differ with the essayist in one point, and that is that the element of time is not a factor in the ill effects of the anesthetic. He says it makes no difference whether the patient is under the anesthetic two hours or one hour. All experiments show that the effects of the anesthetic are in direct proportion to the length of time in which the anesthetic is given. You can kill any animal by keeping it under an anesthetic for a certain length of time, even if the anesthetic be discontinued.

Dr. H. G. Wetherill (Denver, Colo.): In any discussion of general anesthetics in which chloroform is mentioned, the opportunity should not be lost to impress anew upon the profession at large the latent dangers of chloroform. I had intended to write a paper on this subject for this meeting, not because there is anything particularly new in it, but because I believe that the time has come when chloroform as a general anesthetic ought to be absolutely abandoned, and that we should all do what may be in our power to stop its use, everywhere and under all circumstances. I say this deliberately.

Our present knowledge of the late poisonous effects of chloroform as originally pointed out by Guthrie of London, and brought to the general attention of physicians by Stiles of Edinburgh and by Bevan and Favill, in this country, is such that chloroform is already almost driven from our hospitals as a general anesthetic; and it should be driven out altogether, for, notwithstanding the enormous primary danger of chloroform in comparison with ether and other anesthetics, we now know of the late poisonous effects it produces and that death from it may ocur two or three or more days after its administration. In the last year or two our attention has been directed to the fact that chloroform is dangerous in those fields in which we have heretofore regarded it as particularly safe. I refer to the use of chloroform in obstetric work. I saw at the Johns Hopkins Hospital last June the specimens from a maternity patient who died from late chloroform poisoning, exhibited by Dr. Williams. We now know too that it is a highly dangerous anesthetic for children on account of its late poisonous effects. That the administration of chloroform to women in labor produces hemorrhage in the new-born is also now known to us.

If chloroform is primarily highly dangerous, if also highly dangerous on account of its secondary late poisonous effect, if it is going to kill women in labor and kill the unborn and new-born child, the time has come when it must be absolutely abandoned as a general anesthetic.

Dr. A. W. Abbott (Minneapolis): We have been making some experiments in reference to ether as compared with nitrous oxide, and for two or three years I have kept a record of these cases, in regard especially to albumin and casts. In many instances we have been able to secure the urine in from ten to fifteen minutes after the anesthetic was begun, and have found albuminuria and casts in it. We have not used chloroform, so I cannot speak about it; but in the use of ether in all cases there was at least a slight albuminuria produced. It was not in one per cent of the cases or in ten per cent, but everyone had a slight albuminuria. As a general thing, the casts began to show in the urine taken in the course of an hour.

My experience corresponds with what Dr. Harris has said as to the amount of injury to the kidney, it being in proportion to the length of time the anesthetic is given. The longer it is given, the more albumin will be found and the more frequent the casts. I do not believe there can be any mistake about this. We have the records of about 300 of these anesthesias, and I believe what I have said is absolutely correct. This applies in a minor degree to

nitrous oxide with oxygen, of which we have used a great deal. It is followed by the same disturbance, and why should it not be? We have poisoning of the system in the same way, practically with the same results, except we do not get so much; and we keep up the condition of the patient better by the oxygen, so that it should be borne in mind that, whatever anesthetic we use, we are going to have kidney complications. That is one reason why every operation should be completed as quickly as possible, thereby reducing the period of anesthesia.

Dr. S. C. Beede (Davis City, Neb.): There is nothing that succeeds like success. We believe in ether anesthesia because it gives the best results. Yesterday I stated that there were only two cases reported in the literature at my command of the successful treatment of irreducible invagination of the bowel in a child under one year of age. Conversation with a number of the members since that time has confirmed that statement. These are the only two cases reported. Both of them were operated on under spinal anesthesia, and I am glad to know it is recognized by the members of the Association, that, however limited the field of spinal anesthesia may be, there is still a field for it.

Dr. L. L. McArthur (Chicago): The Chair would like to make a remark in regard to the toxicity of novocain and cocain, and would call attention to the fact that it takes seven times as much novocain per kilogram of body-weight to kill as it does of cocain.

So far as the anesthetic properties are concerned, they are practically identical. Therefore, if it takes seven times as much of the remedy to cause the same amount of poisonous action, it is a better preparation than cocain. As it is only a substitution derivative of cocain, it is better to use it than cocain itself. It is the remedy which is practically universally used on the Continent and in England, instead of cocain, on the basis of its toxicity.

Dr. Andrews (closing the discussion): It is certainly valuable to the country practitioner to read a paper on this subject before this scientific body, and then have such valuable discussions as we have listened to on it; and I am very much obliged to the gentlemen who have discussed my paper.

In regard to the danger of anesthetics, especially of ether anesthesia; that the danger is in proportion to the time the patient is under its influence, as stated by Dr. Harris and by Dr. Abbott, I am very willing to concede to their opinion as worth

much more than mine, but it has not been my teaching, and it does not quite agree with my own experience. I have noticed some patients who were kept under an anesthetic for a long time, say two and a half or three hours, who did not rally well and did not do well. Again, I have noticed other patients who were under ether anesthesia not to exceed an hour or so, who did as well afterwards, and the danger seemed to be no more than if the time were less. But those who have made a more accurate study of this than I have must certainly know. I assume in connection with Dr. Abbott's experiment that he always examined the urine before the operation. We have an excellent laboratory in connection with our hospital, it being a branch of the state laboratory, and it is my practice always to examine the urine carefully before any operation, and I have been surprised to find in how many cases there is a slight albuminuria in adult people,—not much, but some albumin in the urine where we would not suspect it at all.

In regard to the use of novocain instead of cocain: I am quite willing to concede, and, indeed, I can well understand, that it is the trend of the profession now to use it as being less dangerous than cocain. Although I have used cocain anesthesia quite a good deal, I have never had an untoward result from its use. Two years ago I was quite enthusiastic over spinal anesthesia. I have never had any unfortunate results or any reason to discontinue using it; but, as I have stated in my paper, the danger of sulphuric ether, if properly administered, is so slight, and its advantages over spinal anesthesia are so great, that spinal anesthesia never can come into very general use. One great disadvantage of spinal anesthesia is, although there may be some unpleasant effects, the drug is here to stay, and when it is used on a patient it is there until it is by nature eliminated from the system. Under ether anesthesia the anesthetist can withdraw the administration of the drug at any time, if it becomes necessary or wise to do so.

THE GROWTH, THE DEATH, AND THE REGENERATION OF BONE

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Radical differences of opinion have always characterized the teaching of the theory of the growth and regeneration of bone. But one factor in the process appears to have been unanimously conceded a place of great importance. The embryologists, the anatomists, the physiologists, the surgeons, and the pathologists have agreed that the periosteum is not only a source of nutrition for the bone, but that it possesses definite osteogenic properties; that from the periosteum the osteoblasts have their origin, in part at least, for the formation of the bones of the fetus; and that the process for the regeneration of bone to replace portions destroyed by disease originates and develops largely in and from the periosteum.

In a recent article one of the foremost American surgical anatomists of today says: "In the new-born and in young individuals, the periosteum is composed of three layers, the outer layer consisting mainly of rather coarse white fibrous tissue, a middle fibro-elastic layer, and an inner osteogenetic layer. * * * * In the adult, the osteogenetic layer has practically disappeared, leaving here and there a few of its cells; a factor that must be reckoned with in every effort at osteal transplantation or grafting."[1]

Another well-informed and progressive surgeon, in an address just published, says: "Osteogenesis

resides in the lower layers of the periosteum, the marrow and the soft structures within the Haversian canals and lacunæ."[2]

Now comes Sir William Macewen, of Glasgow, with a little book[3] in which are detailed many observations from elaborately planned and executed experiments, which, if substantiated and confirmed, completely upset our former views and necessitate a revision and correction of all the standard text-books on anatomy, physiology, surgery, and pathology.

Macewen not only denies to the periosteum an osteogenic function, but assigns to it a function of a diametrically opposite character, namely, that chiefly of a "limiting membrane" for the osteoblasts to prevent their overgrowth into the soft tissues.

With reference to these functions of the periosteum he concludes:

It may be deduced from the foregoing observations and experiments that diaphyseal bone is reproduced by the proliferation of osteoblasts derived from pre-existing osseous tissue, and that its regeneration takes place independently of the periosteum. *The periosteum is not essential to bone-production.* Osseous tissue can pass through all the phases of its life, from its embryonic to its mature form, without the influence of or contact with this tissue.

The periosteum is of great use in limiting within specific boundaries the distribution of the osteoblasts, and preventing them during their evolutionary period from being scattered into the soft tissues, where their presence would be prejudicial to the function of these parts. In the loose areolar tissue existing between the periosteum and the bone the osteoblasts find nutriment for their growth and space to generate, free from undue pressure. While not under-estimating the periosteum as a limiting and protecting membrane, of great use in physiological and pathological conditions, there are no data to indicate that it can, of itself, secrete or reproduce bone. *It has no osteogenic function.*"

The acceptance and practical application of this truth, if truth it be, must materially modify the sur-

Fig. 1. Specimen from young elk showing exuberant callus caused by lacerated periosteum and unrestrained motion between the fragments.

Fig. 2. Sequestrum from Case 1. Diaphysis of fibula removed from its periosteal envelope. Complete restoration of the diaphysis followed the removal of this sequestrum without union at point of fracture near its lower end.

Fig. 3. Case 2, Nov. 24, 1911. Skiagram through plaster dressing, showing Mosetig-Moorhof filling in space left after the removal of the sequestrum. Note the broad encroachment of the filling on the diaphyseal cartilage and how the filling was pushed away from it in the process of repair, as indicated in the subsequent skiagrams. Front and lateral views. I am indebted to Dr. S. B. Childs for this and the following skiagrams.

gery of bones. A new point of view is presented from which, for example, ununited fractures and so-called provisional callus must be considered. The periosteum acting as a barrier to bone-growth would, of necessity, be a factor in causing non-union if it intervened between fragments; and when extensively lacerated or destroyed, or when its function as a limiting envelope is impaired by a failure to immobilize the fragments, the formation of excessive callus through overflow of the osteoblasts into the surrounding soft parts would be inevitable.

All surgical teaching and practice based on the theory that reconstruction of bone depends on periosteum, would have to be revised and rewritten.

The transplantation of bone, *en masse,* without periosteum, already adopted and successfully practiced in human subjects, would be justified, and confirmed in its empirical position. Bone-proliferation and cellular growth circumferentially from grafts *en masse,* or from bone-chips and shavings likewise finds logical substantiation, which confirms the observation of actual results in men and animals.

The theory that such a graft "is, *per se,* not *osteogenetic,* but *osteoconductive,*"[1] must also be held subject to revision.

Macewen's comment regarding this phase of the subject is as follows:

> Small diaphyseal grafts, when placed in a gap in the continuity of bone, show active proliferation from the whole circumference, each piece being an ossifying center from which osseous tissue is thrown out sufficient to fill the gap between the various fragments and to unite them together along with the two ends of the divided shaft. Under suitable surroundings the vegetative capacity of the osteoblasts is at least as great as any of the tissues in the body, and bone-proliferation takes place from its whole surface circumferentially, while epithelium, for instance, proliferated only on the flat.

As an example of the eccentric growth of bone through rents in its periosteal envelope in an untreated fracture, the writer presents a specimen from a young elk he killed in Colorado in 1892. The unconfined osteoblasts distributed through the periosteal rent by the constant use of the animal's leg have produced a large amount of exuberant callus in addition to that required to unite the fragments. (Fig. 1.)

Another striking example is the formation of a bony wall for a false aneurism where the ends of the fractured bone have at once lacerated the limiting periosteum and a large artery. The osteoblasts swept from the bone through the rent and carried by the blood-stream to the side wall of the aneurism form plates or placques of bone upon it, though they be entirely disassociated from both the bone and the periosteum.

The well-known importance of the epiphysis and of the plates of cartilage lying between the epiphysis and the diaphysis in their relation to the growth of the diaphysis is experimentally confirmed, as is also the protection afforded by this cartilage against the diffusion of infection.

Once the epiphyseal plates of cartilage are well formed and complete, they become of service against the advance of pathological products, as they then form a barrier to disease emanating from either the epiphysis or the diaphysis. In this way a pyogenic process emanating from the medulla of the diaphysis is prevented from spreading into the epiphysis, and consequently the joint is saved from invasion; and on the other hand, a tubercular process originating in the epiphysis is barred from encroaching on the diaphysis.

This limiting function of the cartilage, which Macewen prefers to call the diaphyseal cartilage because of its more necessary relation to the diaphysis, is exceptionally well illustrated in one of my cases in which, after the sequestrum and pus were removed, the Mosetig-Morrhof bone wax is shown by the ski-

Fig. 4. Case 2, Jan. 25, 1912. The same leg two months later. Filling pushed away from diaphyseal cartilage and discharged from the wound or absorbed so that little remains. Note the rough ends of the tibia and the gap between the fragments.

Fig. 5. Case 2, March 25, 1912. Ends of the fragments becoming smoother and filling reduced in size.

agram to lie in a large mass directly against the cartilage while no infection of the cartilage, the epiphysis, or of the joint occurred. (Fig. 3.)

In an analysis of Sir William Macewen's conclusions every surgeon of experience will review his own work and results, and attempt to determine for himself how far he believes them refuting or confirming Macewen's findings.

Among the important questions to solve is an occasional apparently unaccountable extensive destruction of the diaphysis without any effort at reconstruction, while in another patient equally extensive death of the bone is attended by an immediate attempt at regeneration of the diaphysis, which speedily goes on to the formation of a perfect bone after the dead bone and pus are eliminated.

As examples of these quite different phases of destructive osteomyelitis I beg leave to refer to two cases. I operated on the first patient and reported the case[4] many years ago in the *Journal of the Trenton Natural History Society* for 1886-1887. The second case I saw within the past year and still have it under observation.

The sequestrum from each case is presented. From the second case the skiagrams showing the primary gap and the steps of the reparative process are also presented. I am indebted to Dr. S. B. Childs for these beautiful skiagrams.

Case 1.—During the winter of 1884 a child aged four had a fracture of the lower end of the fibula about an inch above the epiphysis. The nature of the injury was not recognized by the family. Cold wet cloths were applied, and the child was allowed to hobble about the house for a week, when the leg was immobilized on a splint. A fluctuating swelling was found over the seat of the fracture, which was

opened, and more than an ounce of pus was evacuated. No union of the fracture occurred, and the opening drained freely for three months, when a red spot presenting deep fluctuation was found over the upper end of the shaft of the fibula. This infected area was incised, and upon examining the bone through the incision its denuded surface could be felt. It was apparent that the shaft of the fibula was dead almost from one epiphyseal extremity to the other.

The end of the bone was caught with forceps through the lower incision, and with a slight twisting motion the shaft of the fibula was easily withdrawn from its periosteal sheath.

I herewith present this sequestrum showing the erosion produced by the progressive osteomyelitis. The upper end shows more erosion than the lower. (Fig. 2.)

A rubber drainage tube of small diameter was pulled through the periosteal sleeve by passing a probe from one opening to the other with a bit of string attached. Through this tube, the sides of which were perforated, the suppurating sinus was kept free from pus.

After a little time had elapsed it became apparent that the tube was being encroached upon by the centripetal bony growth from the involucrum, and the tube was removed.

Irrigations were continued as long as the sinus remained patulous. It closed, however, with great rapidity, and the ultimate result was a complete restoration of the shaft of the fibula, though no union occurred at the site of the original fracture near the lower end of the bone.

Case 2.—This was a fulminating osteomyelitis of the left tibia in a boy of six. In August, 1911, the abscess was incised and drained by another surgeon.

Fig. 6. Case 2, July 1, 1912. Remaining Mosetig-Moorhof filling small and in center of the head of the tibia. The ends of the fragments smooth and pointed. The black square is the shadow of a small piece of metal. It indicates the opening of a sinus leading down to the filling.

Fig. 7. Case 2, Sept. 13, 1912. Showing persisting gap in tibia and sharp points of fragments. Lines indicate points at which freshening of the fragments was contemplated.

In November of the same year the patient came under my care and was sent to the Children's Hospital of Denver.

It was apparent that there was extensive death of the bone involving the shaft from near the upper epiphysis to the junction of the middle and lower thirds.

On November 21, 1911, he was operated on, and the dead bone removed. The necrosis extended to the upper diaphyseal cartilage, as is plainly shown by the shadow of the iodoform wax used to fill the space. *The periosteum was not destroyed* and was pushed aside to receive the bone wax. The periosteum and the muscles were then stitched over the wax with fine catgut, and the skin closed over all with small openings to provide for drainage.

The general health of the boy improved at once though the sinus discharged more or less pus, and portions of the wax were pushed out by the growth of the granulations going on beneath it.

The skiagrams made from time to time and herewith shown illustrate the gradual disappearance of the wax and the smoothing of the ends of the fragments.

After many months had elapsed it was evident that no union of these fragments could occur without intervention.

On July 1, 1912, skiagram No. 3367 was made showing a small portion of the bone wax retained in the middle of the head of the tibia, the shadow of the little square below it indicating a bit of metal placed in the mouth of a small sinus on the surface. Later the same day this sinus was spooned out thoroughly, the remainder of the wax removed, and the sidewalls of the cavity containing it thoroughly scraped

and sterilized by an application of pure phenol (carbolic acid). Prompt healing of the sinus resulted.

September 28, 1912, all infection in the leg having disappeared, an operation for grafting a segment from the opposite tibia into the gap was undertaken. The space was opened, and the periosteum denuded from both the upper and lower fragment. The medullary canal was excavated to the depth of half an inch in the upper fragment, and it was my purpose to do the same with the lower fragment, its oblique end, however, appeared so favorable for freshening and splicing the graft on in such a way as to bring broad surfaces together, that this plan for making the union was adopted as is shown by the skiagram taken four weeks after the operation.

Skiagram No. 3646 shows the tibia of the right leg and the site from which the graft had been taken.

The graft was three and one-third inches long, five-eighths of an inch broad, and about three-eighths of an inch thick. *It was taken without periosteum.* The periosteum was regarded as being of more importance in protecting the sound tibia from infection and overgrowth than in the regeneration of the bone of the defective tibia. This slab of bone was driven into the excavation in the head of the tibia and secured by a silver-plated wire-brad five-eighths of an inch long. The lower end was spliced to the freshened oblique surface of the lower fragment to which it was nailed with a similar brad and wired with two loops of phosphor-bronze wire. The muscles were approximated over the graft with catgut, and the skin closed with fine silk-worm gut over all.

The subsequent course of the case was attended by a slight evening rise of temperature. This was explained by a low-grade infection about the lower end of the graft, which made the subsequent removal of

Fig. 8. Case 2, Oct. 25, 1912. Front and lateral views, showing graft from opposite tibia nailed in position to bridge the gap. The graft was driven into an excavation in the medullary canal of the upper fragment and spliced to the lower fragment, as shown in skiagram to the right. Silver-plated wire brads ⅝ inch long were used. The graft was 3⅓ inches long, ⅝ inch broad and about ⅜ inch thick, and was taken without periosteum.

Fig. 9. Case 2, Oct. 25, 1912. The shaft of the right tibia, showing site from which the graft had been removed. At the time this picture was made, four weeks after operation, no defect in the bone could be felt.

the phosphor-bronze wire and the nail necessary. The use of the wire was a mistake. The infection did not, however, impair the vitality of the graft or prevent firm union between the graft and either the upper or lower fragment. Indeed, the union at the lower end seemed to be firmer and stronger at the time the wire was removed, about three weeks after the operation, than that at the upper end, and of the two methods of making the contact I believe that the oblique splice is preferable when the ends of the bones are conical or otherwise adapted to this method.

After eight weeks the tibia was firm, fairly uniform throughout, and *larger in diameter over the graft than the right tibia,* though no defect could be detected by palpation at the site from which the graft had been removed from the right leg.

These two cases and the exhibits bear study in their relationship to the points at issue which are—

1. Has the periosteum, *de novo,* an osteogenic function, or is it in the main a limiting membrane, which prevents overgrowth into the surrounding tissues?

2. Can bone live and grow and proliferate when denuded and disassociated from its periosteum?

3. When transplated to fill a gap, does it become osteogenic or is it merely osteoconductive?

Quoting only those portions which refer to the above items the writer cites from a recent exhaustive summary these crystallized statements from an accepted authority, Dr. John B. Murphy.

Dr. Murphy's article[1] is quoted, not in a spirit of criticism, but from a desire to gratefully acknowledge the splendid original work he has done in this field and because his utterances are regarded as the last word of the progressive workers and thinkers of

America and best exemplify the most advanced American teaching with regard to the subject. Dr. Murphy says:

> In order to achieve the best results in the work of bone-grafting and bone-transplantation the following laws must be observed: The periosteum fully detached from bone and transplanted into a fatty or muscle-tissue bed in the same individual, if he be young, may produce a lasting bone deposit. * * * * * Periosteal strips elevated at one end from the bone and attached at the other, if turned out into the muscle or fat, reproduce regularly bone on their under surface for a greater portion of their entire length. * * * Bone, with or without periosteum transplanted in the same individual and contacted with other living osteogenetic bone at one or both of the ends of the transplanted fragment always becomes united to the living fragments and acts as a scaffolding for the reproduction of new bone of the same size and shape as the transplanted fragment if asepsis is attained. This new bone increases in size as is necessary to give the support required by Nature in the extremity in which it has been placed, etc. * * * The transplanted fragment, no matter how large or how small, is always ultimately absorbed, etc. * * * The graft is, *per se,* not osteogenetic, but osteoconductive. * * * The graft increases in size on the surface as bone increases in size histologically, i. e., by deposits beneath its newly formed periosteum. * * * When the graft is covered with periosteum at the point of contact with the living bone, the Haversian vessels do not penetrate the fibrous periosteum of the transplant and regeneration fails. * * * *If the periosteum of the graft is split into shreds their regeneration through it may take place.* (Italics are mine.—H. G. W.)

Comparing these statements with the conclusions of Macewen we find an expression of a diametrically opposite opinion regarding the osteogenic function of the periosteum and the osteogenic function of bone grafts and transplants, perfect agreement regarding the viability of grafts and transplants, and practical agreement regarding the limiting character of the periosteum, as is indicated in the last paragraph quoted.

Macewen's experiments invariably failed to grow bone from the detached periosteum and quite as in-

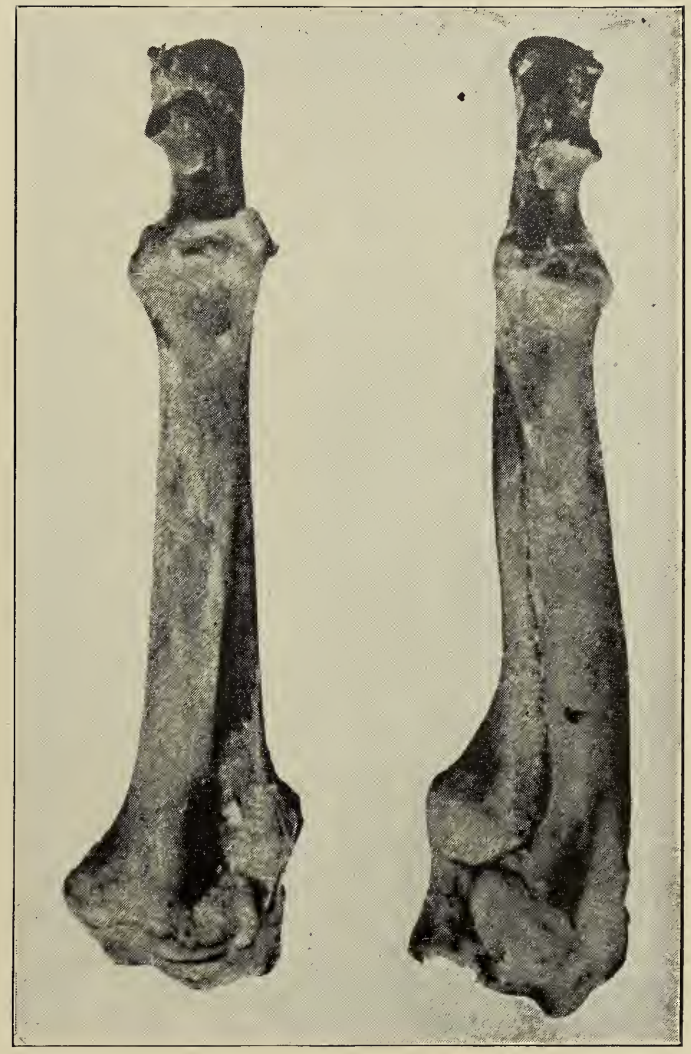

Fig. 10. The radius transplanted from one dog to another without periosteum, with perfect restoration of the diaphysis and only moderate overgrowth. (After Macewen.)

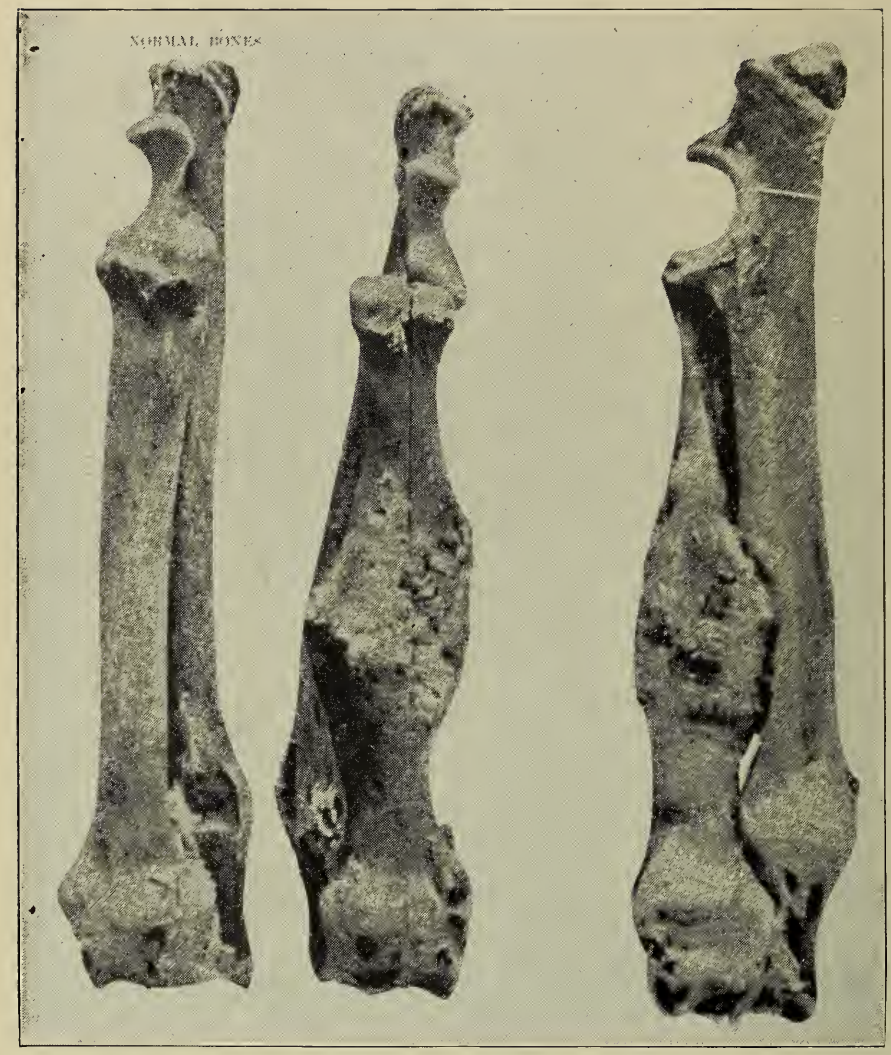

Fig. 11. Marked overgrowth from transplantation of bone in the form of shavings. The proliferation from many foci is exuberant and sufficiently aggressive to erode the adjoining normal bone. On the extreme left are normal bones given for comparison. The bones on the right are the same as those in the middle viewed from the side. (After Macewen.)

variably succeeded in procuring rapid new growth and active proliferation from transplants *en masse* or from shavings, *the more abundant proliferation always coming from the multiple small grafts,* which is not in accordance with the statement that "the graft increases in size on the surface as the bone increases in size histologically, i. e., by deposits beneath its newly formed periosteum," nor with the assertion that such a graft "is not osteogenetic but osteoconductive." Furthermore, the writer has found no authentic record of any instance, experimental or clinical, in animals or in man, in which periosteum detached from its bony contact has produced bone, if no bone-cells or osteoblasts were carried from the bone with it.

For twenty-eight years the author has thought of Case No. 1 as a typical illustration of the power of the periosteum to originate and reproduce diaphyseal bone under favorable conditions.

If, however, Macewen's theory is correct osteoblasts were early thrown out from the diaphyseal bone through and by the aid of the local hyperemia occasioned by the primary infection. These osteoblasts were deposited in the subperiosteal areolar space close to the lower layer of the periosteum. From this point they proliferated, multiplied, and grew to form the involucrum and ultimately reproduce the diaphysis, *in toto,* after the removal of the dead bone; and the periosteum had nothing to do with originating the bone-reproduction. Doubtless the circulation necessary for the growth of the osteoblasts and for their proliferation was received in part from the periosteal vessels, but in part from the end-contact of the involucrum with also in part from the end-contact of the involucrum with the diaphyseal cartilage, but viewing the history of the case from Macewen's point of view and in the light of his conclusion, there is nothing to convince one

that the bone grew from osteoblasts originating in the periosteum, and nothing to disprove that the primary hyperemia did not cause their eccentric deposit in the subperiosteal areolar space from the diseased diaphysis before the destruction of the diaphysis was complete.

The second case presents an altogether different set of conditions, for in this instance the diaphysis of the bone was destroyed by a fulminating osteomyelitis throughout nearly two-thirds of its length *and its total circumference,* and, though the periosteum was not destroyed, no continuity in the restoration of the diaphysis occurred or, so far as could be seen at the time of operation, had been attempted by the periosteum. Indeed, it appeared that the pointed ends of the bones had been covered by the collapsed infolding of the periosteum so that the growth of each fragment toward the other had been arrested by the periosteum and union so prevented, all of which points toward a limiting function rather than an osteogenic function for the periosteum.

This observation would appear to cast grave doubt upon the osteogenic function of the periosteum if like results were noted in a sufficient number of similar cases.

In this case, too, the extremely rapid *overgrowth* of the new diaphysis would tend to confirm Macewen's claim for osteogenesis rather than osteoconductivity in the graft; and as the formation of the connective-tissue envelope for the new diaphysis would require time to reproduce the new periosteum over and around the bone, overgrowth would be favored, provided, of course, that Macewen's theory of the function of the periosteum as a limiting membrane is correct. Other such observations in sufficient number will confirm this contention.

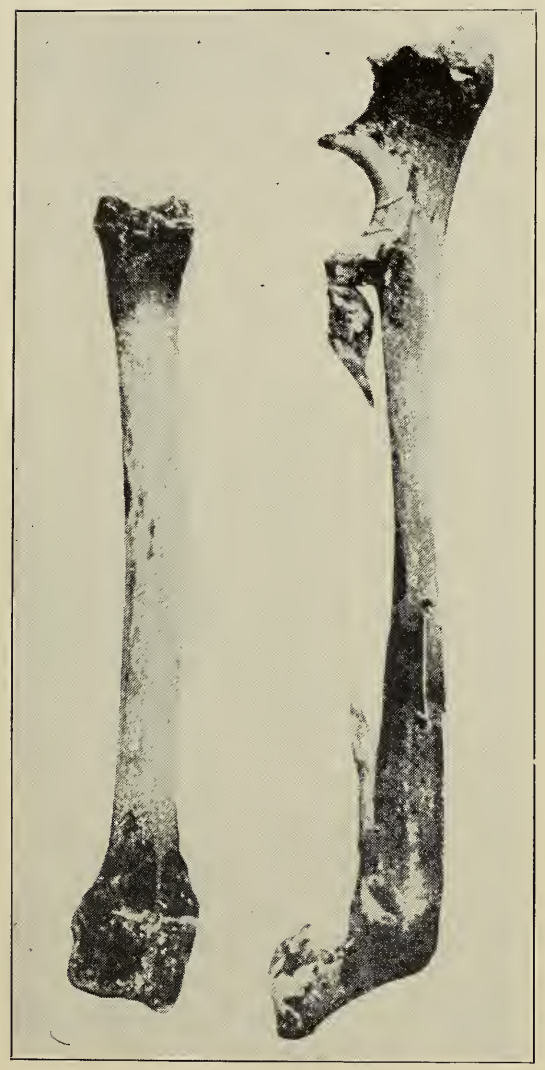

Fig. 12. Showing total failure of periosteum to produce bone. The radius was subperiosteally removed and the periosteum carefully preserved. The styloid process of the ulna has been bent from pressure and an osseus nodule—about the size of a barley grain—projects from its extremity. (After Macewen.)

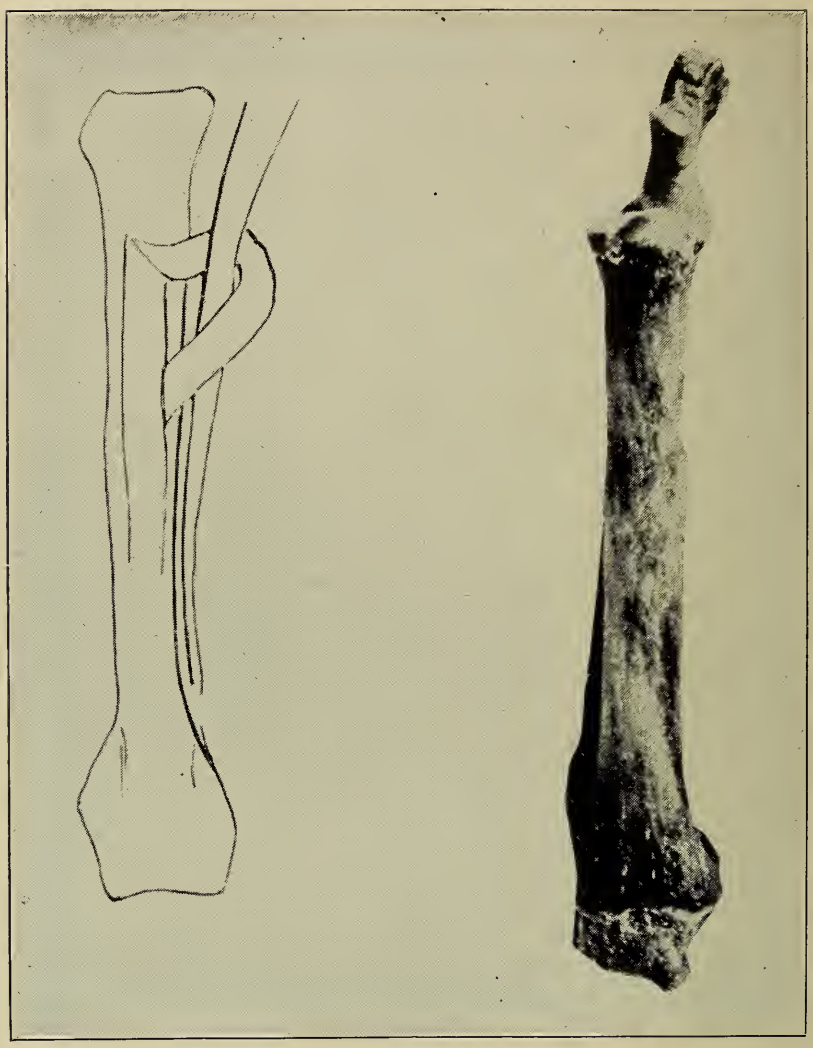

Fig. 13. Flap of periosteum transplanted from the soft tissues. No bone was formed from such transplanted periosteum in any of Macewen's experiments. A slight overgrowth of bone occurred on the shaft of the radius from which the periosteum had been removed. On the left is a diagram showing flap of periosteum raised from bone except at proximal extremity, encircling muscular fasciculus and stitched to border of intact periosteum at lower end. On the right is shown a node-like increase of bone on shaft of radius at the part from which the periosteum was removed. (After Macewen.)

How extensive destruction of the diaphysis may some times occur without effort at reconstruction and how reconstruction is promoted under other conditions are explained by Macewen as follows:

In acute osteomyelitis the infection is at first located in the medulla. In consequence of the great hyperemia induced beyond the area of infection, stimulating rapid tissue-change and hyperplasia, the bone-cells which are beyond the area of pyogenic invasion are converted into their embryonic condition and proliferate with great rapidity. Osteoblasts accumulate in the Haversian canals, and, owing, *inter alia.* to the centrifugal blood-pressure, are extruded on to the surface of the shaft, where they infiltrate the meshes of the loose areolar vascular tissue which exists between the periosteum and the bone. The meshes of this layer become charged with osteoblasts, from which fresh layers of bone ultimately form. If the pyogenic invasion be progressive, the shaft may be involved and die; but, prior to this necrosis, the osteoblasts which have escaped into the periosteum are preserved from destruction. If in a case of osteomyelitis the periosteum be removed from the bone at a very early stage, osteoblasts will still be poured out on to the surface of the shaft, and form there a layer of fresh bone between the muscles and the old bone. This has been directly observed on several occasions.

This is the usual course in osteomyelitis, but it does not always follow in this way. Occasionally, necrosis of the whole shaft occurs without reproduction of bone under the periosteum. This may arise from one of at least two causes. The first occurs when the pyogenic invasion is more virulent, rapid and extensive, causing blockage of the vessels, not only of the medulla, but also of the shaft, producing necrosis without a preliminary period of hyperemia, and consequently before regenerative tissue-changes have had time to ocur in the shaft. The second, where the main nutrient vessels of the shaft at a very early stage become thrombosed by pyogenic invasion, the blood-supply of a large portion of bone is cut off, and necrosis occurs before proliferation within the bone can take place. In some such cases the periosteum participates in the destructive process, but it does not do so in all. Owing to its separate blood-supply, it is possible for the periosteum to live and it sometimes does so apart from the bone. In such a case, however, there is no regeneration of osseous tissue, there having been no osteoblasts regenerated from the bone and thrown from the shaft into the subperiosteal areolar tissue before necrosis set in.

This explanation of the death of the diaphysis *in toto* without any attempt at regeneration on the one hand and of a hyperemic deposit of osteoblasts in the subperiosteal areolar space to form new bone on the other, fits in perfectly with what we now know regarding the action of emboli and infarcts, and of the effects of local hyperemia elsewhere in other tissues.

We are prepared, therefore, to accept this theory in accounting for both the destructive infection and the subsequent regeneration of bone under circumstances favorable to either.

We have seen elsewhere the deadly embolus instantly and irrevocably destroy every shred of tissue depending upon the blocked vessel for life, and we have seen the slower invasion of an equally virulent infection met by a resisting and regenerating wall of leucocytes thrown out about it through the agency of a local hyperemia with early arrest of the infection and regeneration of the tissues. Why may not the same principles apply and operate in the same way in bones?

The growth, the death, and the regeneration of bone have long been fearfully misunderstood if the theories of Sir William Macewen regarding these processes are correct. He presents them clearly and logically, and proves them by numerous ingenious experiments upon animals and clinical observations on patients. He draws no conclusions he has not verified, and his "comment" is so temperate and conservative that it carries conviction with it.

Viewed from Macewen's standpoint and in his light, I find the two typical cases quoted in this paper confirming my other observations under similar conditions, and I am quite ready to accept the new doctrine and adapt myself to it in its actual application to the day's work.

Should those with better knowledge of the subject and larger opportunities for practical verification of the doctrine, both experimentally and clinically, confirm the conclusions of Sir William Macewen, much of the literature of medicine will have to be rewritten. The teaching of surgical anatomy and pathology must then be conformed to the new gospel and the practice of bone surgery be revolutionized.

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DISCUSSION.

Dr. James E. Moore (Minneapolis, Minn.): This is a very valuable, timely, and interesting paper, and I am very much pleased to add my mite to the discussion.

The growth and development of bone has been the subject of a great deal of discussion recently. I have been familiar with Macewen's work for twenty-three years, and was mostly impressed with it nineteen years ago when I met him in Rome at the International Congress and talked the matter over with him at that time. Dupuytren, Cruveilhier, Flourens, Heine, Virchow, and Gegenbauer have all held that bone could be produced from periosteum or other connective tissue. Rokitansky and Hofmokl claimed that the endosteum contributes an essential element in the production of bone, but ascribed considerable powers to the periosteum in this same connection. Senn definitely states that provisional callus is the product of tissue proliferated from the periosteum, while the permanent callus is formed entirely from medullary tissue.

The work of Macewen has made clear the reasons for this difference of opinion among various observers. The rules laid down by him are clear and definite, and have been confirmed by the results obtained in each animal experiment on bone undertaken in the laboratory of the University of Minnesota by Dr. J. F. Corbett.

The principles of Macewen are essentially as follows:

1. Bone can be produced independently from preëxisting bone without the aid of the periosteum.

2. The periosteum alone of normal unirritated bone cannot produce new bone.

3. The periosteum serves as a limiting membrane to the overgrowth of new bone.

4. The periosteum serves as a blood-carrying framework on which the vagrant osteoblasts may find refuge and nourishment.

5. Bone deprived of its periosteum gives rise to growth beyond its natural limits, that is, bony masses or tumor.

6. Fragments of bone transplanted into the place formerly occupied by bone and deprived of periosteum, take on the character of unlimited growth.

7. The epiphyses have very poor regenerative qualities.

8. The epiphysial cartilages have great reproductive power.

One of the experiments done in our laboratory substantiates the principles laid down by Macewen. A channel was cut in a bone and on one side of the channel the periosteum was entirely destroyed, while on the other side the periosteum with the attached fascia was drawn down into the shaft of the bone, and anchored there. As a result of this there was produced after a period of time new bone on the interpolated periosteum. This is explained in the following manner: The bone was constantly irritated by the piece of fascia introduced into the channel, and as a result of this irritation there were discharged to the periphery a large number of osteoblasts. On the other side, where there was no periosteum, the osteoblasts escaped and were lost in the tissues. The periosteum on the inner side served as a rallying-point for the escaped osteoblasts. They accumulated on this membrane, were nourished by it, and produced considerable bone. This tendency of the osteoblasts to leave the bone and seek a resting place on the periosteum is only encountered in the presence of irritation and traumatism. The general principle in biology, that all cells come from like cells, would be violated if the periosteum, which is a fibrous membrane, were to produce bone-cells.

I have, for a long time, been convinced of the correctness of Macewan's contention as to the function of the periosteum, and have been teaching accordingly, but as to whether transplanted bone has real osteogenetic powers, I am not so certain. If the biologic principle above mentioned is true, the question naturally arises, how are we to account for bony deposits in fibroids, muscles, and other soft tissues? The most likely answer is,

that those deposits are not true bone, but something resembling it. Since the question of the power of the periosteum to produce bone has been raised, it calls attention to what we have believed in the past as to the power of the endosteum to produce bone. Macewan says it has no such power. We know that in times gone by we were careful to preserve the periosteum so that it might produce bone; and we have been careful to preserve the medulla and endosteum, so that they might produce bone. The endosteum is the same type of membrane as the periosteum. If the periosteum has no osteogenetic powers, have we any good reason to believe that the endosteum has? I can remember very well my early experience in operating upon ununited fractures, feeling it incumbent upon me, not only to preserve the periosteum, but to drill the ends of the fragments if the sawed off part did not enter the medulla. I would drill practically through so as to gain the osteogenetic powers of the medulla. Of late I have not done that, and have secured bony union.

Dr. Daniel N. Eisendrath (Chicago): We have been wandering around for many years, especially during the last five or six years, in the dark on the subject of what is the cause of new-bone production, especially in the case of transplants, so that the paper of Dr. Wetherill in calling our attention to the views of Macewan is exceedingly timely and valuable, at least to me. I know of no better confirmation of the new formation of bone from bone-tissue itself than a case which I unfortunately did not have the opportunity to speak of yesterday in connection with Dr. Percy's paper.

I have had two bone-cysts. One of these cases has been reported by Dr. Harry Sherman in *Surgery, Gynecology and Obstetrics.* The patient, a boy, was originally treated by me in the Michael Reese Hospital on account of a spontaneous fracture. There was this condition found in the bone at the time of the operation. We had the typical appearance to which Dr. Percy has called attention in the *x*-ray picture, namely, a thin wall of cortical tissue, and the cavity-formation in the bone in contradistinction to the normal thickness of the cortex, as we find it below the points where he showed this yesterday, namely, the normal cortical thickness, as shown in the *x*-ray, is one-fifth or one-sixth its thickness in a typical bone-cyst.

We made a diagnosis of bone-cyst in this boy at first. We opened the thin parchment-like shell and scooped out the tissue to which Dr. Percy referred yesterday, and filled the cavity with bismuth paste. We took pictures of the boy at intervals for

about six months or a year, and the bismuth paste was still there. Two years later I received a telegram asking me to recommend some surgeon, for the reason that the boy had sustained a spontaneous fracture. I referred the members of the family to Dr. Harry Sherman, and he was kind enough to give a full report of the case in connection with the article. He has a series of *x*-rays, which show in succession the new-bone formation to perfection. He found at the time the boy came under observation the bismuth paste in the cavity unchanged. He simply opened the cavity, as I had done, scraped out the bismuth paste, filled the cavity with salt solution, and closed it in the manner Dr. William J. Mayo recommended; and a series of *x*-rays gradually showed the development of bone from the thin cortex left until the entire cavity was replaced by bone. I mention this case particularly because every now and then we see reports of cases of bone-cyst in which very radical measures have been undertaken. I recall one case presented at a clinic in which the radical measure of resection of the entire bone was undertaken, transplanting another piece of bone to take the place of the bone-cyst. If we would only remember that the osteogenetic power of the remaining bone tissue is sufficient to take the place of any cavity, provided there is no infection present, we need not resect any of these bone-cysts.

In regard to the use of bismuth paste: I believe in many of these cases, especially if there is any infection, we are defeating the primary object by injecting into the cavity bismuth paste, because, as I have seen it recently, the bismuth paste acts as a foreign body, as in this case, and one or two weeks ago we found the osteomyelitic focus filled with bismuth paste, with no effort made on the part of Nature to get rid of it.

These cases simply confirm the theory of Macewen, as explained here by Dr. Wetherill. Every one who has had occasion to operate on cases of osteomyelitis sees an effort on the part of Nature towards a thickening of the periosteum. What we do see is this: Perhaps we will find bone scattered at various places, as in the last case I spoke of, with the entire shaft of the bone, the medullary cavity, and cortex continuous with each other, simply a replacement of the entire medullary cavity by solid bone-formation or cystosclerosis, which is a confirmation of the osteogenetic power of the bone tissue itself.

Dr. Charles H. Mayo (Rochester, Minn.): Mr. Stiles of Edinburgh teaches that the periosteum plays an important part in the production of bone, while Macewen of Glasgow believes that the bone does all the work. In Edinburgh one may see

Stiles performing operations on children with inflammatory diseases of the bones. He splits the periosteum from top to bottom, shells it from the bone, saws the bone in two, gives at a quick pull, and takes out the two halves, leaving the epiphysis at each end. He then brought in several children to show the different periods of development of the bone. In Macewen's clinic a cat was exhibited in which macerated bits of bone had been injected beneath the skin with a long needle. The cat was covered with bone tumors. In every place where the bone was implanted it had grown, but in no case in which periosteum had been planted beneath the skin did bone develop.

Both surgeons are more or less correct in their theories. In the absence of suppuration there will be very little growth or regeneration of bone, but in the presence of suppuration and consequent irritation bone will develop from the periosteum.

Dr. Arthur T. Mann (Minneapolis, Minn.): Sometime ago I did some experimental work on fractures in guinea-pigs, and I have not in my own mind decided whether the periosteum will produce bone or not. Some interesting points, however, were observed. In a fracture in the guinea-pig, with the periosteum and central bone-cavities, we noticed this: There is almost no callus-formation at the end of the cortical layer, but there is some. There is a good deal of callus-formation in the inner layer of the periosteum and a great deal from the central structures. These grow together until they coalesce, making the main callus.

An interesting point here is, that no matter how large the mass seems, the bone seems to develop against the shaft first. It is in larger blocks here, as though the periosteum is putting out the new-bone growing tissue, or as though it were growing up from the bone-shaft. That seems to be true. So far as it goes, it would confirm the statements of Macewan.

In regard to the operative cases where bones are shelled out: We early learned that if the periosteum were normal and the bones shelled out early, a great many cases did not reproduce bone. I remember very well a case which I assisted in operating upon with Dr. Cushing of Boston in which we had an acute osteomyelitis of the tibia, which had lasted one week, and at that time there were sinuses near the epiphyseal line below, and Dr. Cushing determined to split the periosteum and to saw the bone and remove it. This was done. The epiphyseal spaces at both ends were involved, and there were small abscesses throughout the medulla. I remember very well as the knife cut the periosteum it grated, in other words new bone had

already begun to form in the lower layer of the periosteum, so that when the shaft was out there was a layer of new bone forming inside the periosteum. In six weeks the tibia was regenerated with a fresh shaft one and a half times the normal size. It was loose at the epiphyseal lines. A year and a half later it was tight at the epiphyseal line above, and it was slightly movable at the line below.

It seems to me, in thinking this over in the light of what Macewan has said, that the infection had separated the periosteum below early, and it is possible it might have separated the entire periosteum; and if Macewan's statement is correct, that periosteum does not produce bone, it would explain the reason why this strip of periosteum, separated very early, before the osteoblasts became attached to it as they had in the diaphysis, did not produce bone and did not make a firm union at the epiphyseal line.

I think more work must be done on this before we can determine as to the power of the periosteum to reproduce bone.

Dr. Allen B. Kanavel (Chicago): It may be of interest to the Fellows to know that during two years I performed a series of experiments on dogs in relation to splenectomy and fracture of bones, the idea being that we had a number of cases of traumatic injuries with rupture of the spleen and fractures of the bones. Associated with the physiological relation which is known to exist between the spleen and marrow of the bone, it was my desire to determine whether or not splenectomy had any relation to the repair of the bones. It apparently did not. The bones united just as quickly with splenectomy as in those cases in which it was not done. My microscopic sections differed somewhat from those mentioned by Dr. Moore, in that the endosteum of the bone did not proliferate to any marked extent. In a fracture, on microscopic examination of my sections, I would find that the largest amount of growth, by far, came from the portion of the bone in which the periosteum was still attached to the bone; and while we did have some growth from the endosteum and some from the bone of the shaft, as has been mentioned, it was evident the growth bulged out from that part in which the periosteum had still remained attached to the bone. As far as the endosteum is concerned, we would find, possibly in one case in three, considerable proliferation, a bulging out in like manner, but not one-fifth as much as we would find from the periosteum.

As regards the medulla: We find practically no regeneration of bony tissue. It would come out as a fibrous growth and cartilaginous tissue.

Another clinical point that came out was the length of time it took for real bony union to occur, which was a number of months,—six or nine or ten months,—before we would have hard bony union with the deposit of salts.

Dr. Miles F. Porter (Fort Wayne, Ind.): The burden of proof lies with those who assume that cells can beget tissues other than their own kind. It is reasonable to suppose that the so-called deposits of bone at a considerable distance from the normal location of bone may be explained upon the ground that the osteoblasts, for some reason or other, have escaped, and have been carried by the circulation to the point at which these bony deposits are found. It occurs to me that there is no question whatever but that we do find in the thyroid and other tissues of the body perfect bone. These deposits have been shown by the microscope to be true bone. They present all the ordinary histological characteristics of bone. That does not mean that the thyroid has the power of forming bone, but it simply means that in the thyroid the bone-cells, the osteoblasts, which have lodged there have received the necessary pabulum and have continued to grow, and so it seems to me to be the same with the periosteum. Because there seems to be in connection with the periosteal layer a larger growth of bone, it does not follow at all that the bone originated there and came from the periosteum, but that it simply grew in greater profusion or more rapidly there because the environment was better for its growth; yet, after all, bone-cells leading to this growth must have originated from the osteoblasts. We would have to revolutionize our theories if we came to the conclusion that anything else than bone could reproduce bone.

Dr. M. L. Harris (Chicago): This question of the regeneration of bone is a very old one, and it created considerable furor about twenty-five years ago when a number of young German experimenters tried to determine whether a button of bone removed from the skull and replaced would live as bone or would simply form a structure for new bone. That was discussed pro and con. Experiments were repeated, and it was finally settled that the button of bone did live. I do not think that this question is entirely settled; in other words, there are a number of confusing points which must be cleared up before this matter can be dismissed.

One case which I had recently is very interesting as bearing on this subject. It was a case of fracture of the leg in which four inches of the tibia was lost entirely. The fibula was fractured in this way (indicating). This was a compound frac-

ture. The leg would have to be shortened four inches or a graft taken from another portion of the body. I took the fibula, cut it off, and turned it over, and put it into this place.

Here is a case of acute fracture in which no inflammation had existed. I did not expect any regeneration at the fibula. The case was not treated with that end in view. The first plate I took showed a thin streak of bone the entire length, and that progressively increased until now we have a new fibula.

A Member: Did you remove the periosteum?

Dr. Harris: No, it was left.

A Member: How many days after the injury was the operation?

Dr. Harris: The operation was done four or five days after the injury. On the other hand, let me refer to the work of Hoffmann in transplanting large pieces of periosteum, not with a view of osteogenesis, but to make movable joints. In place of putting in fascial flaps, he has put in large flaps of periosteum taken from the tibia. These periosteal flaps covering over the joints have not reproduced bone, showing the periosteum apparently has not regenerated bone. That the transplanted pieces of bone will regenerate there is no question, as Albee's work of putting in the bone graft in Pott's disease shows. It unquestionably grows in length. He pust a piece of bone in, and as the child grows this graft grows, and the kyphosis growing in length when he did not want it. It grows in all directions from the bone-graft.

Dr. Charles H. Lemon (Milwaukee, Wis.): Sir Astley Cooper stated that intracapsular fracture of the neck of the femur was incapable of bony repair. During the entire period of his long career, even to the end, he reiterated this statement.

In an effort to ascertain why a man of such ability as Sir Astley Cooper should persist in this statement and reiterate it through a period of thirty years, I made a study of his writings and arrived at a conclusion not thought of by present writers on the subject of fractures and regeneration of bone. His conclusion was that without pressure there is no formation of callus. He stated that, in fractures of the femur at the lower third where there was over-riding of the fragments, he was unable, by any mechanical means at his command, to make an end-to-end approximation of the bones. In the treatment of these fractures he used a leather cuff to cause lateral pressure between the fragments and in this way secured bony union.

Referring to a specimen of an elk's leg by Dr. Wetherill, it

shows beautifully how this bony union takes place. The pressure and irritation caused by the movements of the wild animal from place to place produced the necessary bony formation. The necessity for apposition of fragments combined with pressure is the central thought found in Sir Astley Cooper's letters and lectures of one hundred or more years ago.

Those who have examined *x*-ray plates of bones which have been approximated by the use of the Lane plate, must have been struck by the fact that we always find an entire absence of what is known as provisional callus, whether the plate was taken a day, a week, or months after the operation was made, providing of course the operation was done according to Lane's technic.

I had the privilege of examining a series of experimental Lane-plate operations made on the bones of animals in the majority of which experiments non-union had occurred. An examination of these specimens disclosed the fact that the plates actually held the bones apart, so that the ends did not come in contact with each other at all.

In observing the technic of Mr. Lane, two years ago at Guy's Hospital, I was impressed by the fact that before applying the plate to the fracture he took the greatest pains to make an accurate apposition of the fragments. He split up the periosteum at the site of the fracture for a considerable distance on either side, so that the bone, for several inches on either side of the fracture, was bare, and the approximation of the fracture could be clearly seen. The periosteum with its attached muscle was allowed to fall together over the plate.

If the fragments are thus carefully approximated and the drill-holes made slightly excentric to the center of the eyelet of the Lane plate, the ends of the bones, when the screw is driven in, are gently pressed together, and we obtain direct pressure of one fragment against the other, and it is this pressure that sets in motion the process of repair. Where the fragments are thus accurately approximated no provisional callus forms. Union takes place by the new linear formation of bone analogous to the healing that occurs in the soft tissues in a wound that heals by primary union.

Where fractures are treated by the application of Lane plates, the patient should not be permitted to walk on the fracture, if in the lower extremity, as early as we have been accustomed to permit them under the former practice, where we had usually a large amount of provisional callus.

A fracture which is imperfectly approximated and is not

immovably fixed will always show a considerable amount of provisional callus.

This fact led Mr. Lane to make the statement that "provisional callus is an evidence of bungling on the part of the surgeon."

Dr. Joseph E. Smith (Wausau, Wis.): At the recent Clinical Congress of Surgeons held in New York, Dr. McWilliams, in discussing the theory of Macewan, showed a number of cases in which he had performed experiments in animals and human beings, by transplanting bone without periosteum; and his statement at that time was to the effect that he had in no case been able to secure bony regeneration unless the periosteum was transplanted with the bone.

I mention this simply to show that this matter is still in a considerable state of confusion.

Dr. L. L. McArthur (Chicago): I would like to make a few remarks on this subject. When Rovsing got up in the German Congress and demonstrated that iodoform was neither an antiseptic nor a disinfectant, and that bacteria could grow in its presence, Koenig arose and said: "Empirically, we have found that it is good, that it is useful, and we shall continue to use it empirically, although you say it is neither an antiseptic nor a disinfectant." So in the clinical experience that we have all had. This preservation of the periosteum in cases of fracture has not been arrived at with out hoping that new bone and sections would be reproduced, particularly when that periosteum has been inflamed, and Macewen, twenty years ago, was the one to urge the preservation of inflamed periosteum for the regeneration of new bone. Now we know the osteoblasts are beneath it. Clinically, again, we have had the experience that in a clean amputation a little flap of periosteum of the tibia laid over the end of the bone does not produce new bone, and leaves a zone under there that can be felt for years afterwards, of the same contour as it was before, not producing new bone, but nicely healing the cut ends of bone.

In regard to the taking of grafts for transplantation: It is best, in my opinion, to take a portion of periosteum along with the graft. One portion of the graft will be fed by periosteum, while the other will have its nourishment by imbibition. Why do this? Because in the space of six or eight days a new vascular connection is made with the vessels in the periosteum, and those vessels supply nourishment to the bony graft. Therefore it is a better-nourished graft than one that has to depend upon

the contact and the formation of vessels from the opposite direction.

I would like to ask the essayist if he has any knowledge of the migration of osteoblasts? I do not believe they have any ameboid movement. I believe there is an active reproduction from nuclei in the neighborhood of the osteoblasts and no migration of them.

Dr. Wetherill (closing the discussion): I am very much gratified at the interesting discussion my paper has developed, and the views brought forth. It is particularly interesting that there seems to be a general disposition on your part to accept the views of Macewen, notwithstanding established notions lasting through so many years, that bone grows from periosteum.

Answering Dr. McArthur's question: I would like to say that so far as I am aware there is no power in the osteoblasts to migrate. When a fracture occurs the osteoblasts are swept into the soft tissues by the ciculation through the torn periosteum. Where a large vessel is ruptured by the bone-fragments they may be carried by the blood-stream to the wall of the false aneurism, depositing a bony plate on the wall of the aneurism. It is not an ameboid movement. The osteoblasts get into the blood-current and are conveyed by it.

Dr. Mayo brought up a very important point in connection with the case of Dr. Harris. Dr. Harris tells us it was four days before his operation was done. It is conceivable at least that there were enough of osteoblasts deposited on the under side of the periosteum during these four days to bring about reproduction of the shaft of the bone. It may be better in such cases to wait a sufficient time for a deposit of osteoblasts underneath the periosteum before transplantation is made. This is an important suggestion.

Dr. Eisendrath has made the point that the Mosetig-Moorhof plug may have been a bar to union. That also is a construction of bone. From my limited observation I believe Dr. Eisendrath may be right. In this case the large Mosetig-Moorhof plug may have been a bar to union. That also is an extremely important and practical point.

I cannot agree with Dr. Mayo that this is not an important matter. If we have definite ideas, and our operative procedures are based on certain principles, we ought to get certain results. The case Dr. Mayo has quoted is a definite confirmation of the contention of Macewen, though not intended as such. In saying that, I want to say that, in my opinion, there is no better sur-

geon than Harold Stiles, but is it not apparent that in his pathologic cases there has been ample opportunity for depositing the osteoblasts under the periosteum? If he believes bone develops from periosteum in such cases, is he not deceiving himself as well as Dr. Mayo? Dr. Mayo's citation of Stiles' work is, as I see it, an unanswerable confirmation of the contention of Macewen.

TEMPOROMAXILLARY ANKYLOSIS

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In considering the subject of constriction of the jaw this paper will be limited to those forms of the disease which depend upon changes in the temporomaxillary articulation. Other forms of the malady where the immobility of the maxilla is due to cicatricial contraction following ulceration, sphacelation of the soft structures about the mouth, and other causes, will not be discussed.

Ankylosis of the temporomaxillary articulation is practically always the result of arthritis. The arthritis, in turn, may generally be traced to some of the various infectious diseases.

Arthritis sicca may be an exception to the rule. It is a fact that synarthrosis of the temporomaxillary joint is sometimes caused by the dry form of arthritis. In such cases the mobility of the joint is prevented by the filling up of the glenoid cavity and the production of stalactiform exostoses and even loose bodies of bone in the joint-cavity.

Arthritis and periarthritis of the temporomaxillary joint may be due to extension of an inflammation from the neighboring tissues.

Otitis media, parotiditis, and osteitis affecting the body of the maxilla, may cause an infection of the temporomaxillary joint.

The authorities all speak of scarlatina, rubeola, rheumatism, puerperal fever, typhoid fever, and gon-

orrhea as being capable of causing arthritis and consequently ankylosis of the temporomaxillary joint. According to Heydenreich, in his article on "Diseases of the Temporomaxillary Articulation," in "The System of Surgery" by Duplay and Reclus, traumatic and gonorrheal arthritis constitute the most frequent causes of ankylosis. He cites seven cases of temporomaxillary arthritis, observed by Fournier, due to Niserian infection.

Traumatic arthritis seems to be somewhat frequent. In most of the cases the injury seems to have been caused by a fall upon the chin.

Complete ankylosis of the temporomaxillary articulation is truly a very serious matter. In the preaseptic age of our art it was a far more serious condition to encounter than at present. The unfortunate patient was often obliged to take his nourishment through an aperature created by the extraction of one or two of the upper and lower teeth. The evolution of the surgical treatment of the condition under consideration is an exceedingly interesting subject to study. Many procedures were proposed by the older surgeons, which would appear crude and almost barbarous to the present age of surgical refinement. Some of these rude methods were actually put into practice. There were invented many different forms of wedges, screws, and levers with which to force the jaws apart. Heydenreich credits Rhea Burton with being the first to suggest the treatment of ankylosis by section of the bone near the joint involved. According to the same authority, Carnochan, Diffenbach, and Richet proposed this method for the relief of temporomaxillary ankylosis. Grube, again, according to the same historian, was among the first to section the condyle. He performed his operation with a chisel entered through the buccal cavity. In 1854 Humphrey of Cambridge divided the condyle

through a cutaneous incision. Bottini of Pavia sectioned both condyles through a cutaneous incision. This was the popular operation for several decades. In 1880 Abbe of New York divided the condyles through a skin incision.

With these methods injuries to the facial nerve were of frequent occurrence.

Ollier devised a T-shaped incision which enabled him to deviate the nerve from the field of operation. Koenig made use of an incision similar to that of Ollier. Kummer made his incision along the superior border of the zygoma with a supplementary descending limb in front of the ear. He recommended division of the arch of the zygoma temporarily as a means of better access to the joint. Esmarch conceived the idea of creating a pseudarthrosis in the ascending ramus of the inferior maxilla. The idea occurred to his mind after observing a case where the spotaneous elimination of a sequestrum was succeeded by the formation of a useful false joint. Heydenreich gives Helfreich the credit of being the first to interpose a piece of muscular tissue between the divided fragments of the condyle for the purpose of forming a false articulation. Rochet sectioned the ascending branches of the maxilla. Bennet excised both angles of the jaw with a good result.

Heydenreich says Rizzoli's operation was similar to Esmarch's.

Levrat divided the bone between the last molar tooth and ascending ramus. Kraske advised preliminary tracheotomy to obviate the occurrence of suffocation in the event of vomiting during the anesthetic.

These historical facts have been noted for the purpose of showing the manner in which surgery of the temporomaxillary ankylosis has been developed.

The article by Howard Lilienthal, in the August number of *Annals of Surgery* for 1911, describes a method of operation that appears to me to be a great improvement over previous methods. The merit of his operation consists in easier access to the joint and the obviation of injury to the facial nerve. Lilienthal's method of approach to the field of operation, with Murphy's method of dealing with the ankylosed joint, constitutes a most ideal method of dealing with ankylosis of the temporomaxillary joint.

There is but little disfigurement following the operation. The function result is all that could be hoped for. The patients are able to masticate all kinds of alimentary substances within a very few weeks after the operation, provided, of course, the wound heals without infection.

My patient was Miss Emma N. B., a school-girl residing with her parents at Yates Center, Kansas. She was referred by Dr. West, also of Yates Center, and the regular physician of the family. The child had always enjoyed good health until two years previously, when she was sick of typhoid fever. Her attack of typhoid occurred in the autumn of 1909. She was sick of the fever for some twelve weeks. Her illness was very severe, and she was delirious most of the time. During the last week of her fever she was unable to open the mouth. The attendants were obliged to force the jaws apart to give nourishment. After her recovery she still had difficulty in separating the jaws, and mastication soon became impossible. The inferior maxilla was slightly retracted, and she nourished herself by forcing soft articles of diet up between the upper and lower teeth with her thumb and fingers. It required a good part of her time to nourish herself with food and drink.

She entered my service at St. Francis Hospital, Wichita, Kansas, and was operated upon September

2, 1911. Great care was exercised in her preparation to obviate vomiting from the anesthetic. The right side was operated upon first. According to Lilienthal's method an incision was made beginning in front of the auricle, extending along the upper border of the zygoma and down to the periosteum, and about an inch and a half in length. A descending incision through the skin only was made beginning at the posterior end of the first incision and extending downward in front of the ear and toward the angle of the jaw, and about an inch and a half in length. The triangular flap of skin was then carefully dissected off and turned downward and forward. The zygoma was now sectioned near the posterior and anterior ends of the incision with a small sharp bone-pliers in preference to the Gigli saw. The zygoma was then retracted downward carrying along the attached masseter muscle. The region of the ankylosed joint was now easily accessible. There was complete bony synarthrosis, leaving scarcely a vestige of the articulation. A chisel was used to open up the joint, and a small sharp bone curet was employed to clear away the adventitious bony tissue, operating mostly at the expense of the maxillary condyle. A chisel was then introduced between the condyle and the glenoid fossa, and, using the shelving radix of the zygoma for the fulcrum, the condyle was forced downward to still further destroy the adhesions about the joint. A piece of the temporal fascia was now divided so as to leave the attachment to the inner border of the root of the zygoma undisturbed, and passed across the articulation between the glenoid fossa and the condyle and stitched with fine catgut. The section of the zygoma was now replaced and stitched in place with catgut sutured through the fascia. The skin-flap was adjusted with horsehair sutures. Some mobility of the maxilla could now be observed.

Eleven days later the same operation was done on the other side. The jaws could now be separated widely with but little difficulty. Within two or three days the patient could drink water from a glass. But little restriction was placed upon the patient with regard to the use of the jaw. She was allowed, even encouraged, to eat apples after the lapse of ten days. I had the dentist make a hard rubber interdental wedge to be worn between the teeth for a few hours every day to prevent relapse. The patient still remains cured.

DISCUSSION

Dr. L. L. McArthur (Chicago): I think this paper is worthy of comment and perhaps it is worth while to record three cases that have been referred to me from the dental department of the Northwestern University with ankylosis of the jaw, in which an operation was advised and performed, with some of the modifications that have been suggested, and which resulted in every instance in a good result. I believe, in the first place, it is absolutely unnecessary to make any resection of the zygoma when the external aspect of the joint has been exposed and the contour of the zygoma followed. One generally should begin with a curved gouge, and should chisel away the bony union between the glenoid fossa and the articulation. After having made a skin incision down to and a little beyond the level of the joint anterior to the arch, one will be enabled, by cutting through the skin and turning back a skin-flap, to expose the temporal fascia above the zygoma. When that is exposed, a flap of the temporal fascia is then made and turned back. In turning down this flap some branches of the temporal artery may be cut. There is no danger whatever to the facial nerve, which lies well below the line of operation.

A point which I learned in connection with this bony ankylosis is, that, unless it is carefully studied, you cannot tell whether you have a double or a single ankylosis. By mere chance I operated upon a certain side of what I thought was a double ankylosis to make the operation in two steps, and to my surprise the opposite joint was perfectly free. We may make a mistake unless we are careful to investigate, and may operate on the wrong side first. I believe it can be determined by observation as to whether there is little motion or separation of the ligament.

BONING OF THE THORACIC PRECARDIAL WALL IN CERTAIN AFFECTIONS OF THE HEART: REPORT OF A CASE

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In collaboration with Dr. A. D. Dunn, of Omaha, I am about to publish in the *American Journal of the Medical Sciences* an article entitled "Observations on a Case of Mediastinopericarditis Treated by Cardiolysis (Brauer)." He feels, however, that it would be unwise to let this opportunity pass without briefly drawing the attention of the practical surgeons present to the importance of this operative procedure.

Cardiolysis, or pericardiac thoracolysis (Kocher), has found no favor in America. Why this is so is incomprehensible to me, because the operation is based upon good mechanical principles, is not difficult of execution, nor has it a high mortality. The danger of the procedure is so inconsiderable in proportion to the good to be expected, that every physician should familiarize himself with the diagnosis of the conditions calling for the operation, and every surgeon should, in addition to this, learn the technic.

History.—According to Brauer, whenever, in pericarditis, the heart becomes adherent to the pericardium, and the pericardium to the mediastinum, pleura, and chest-wall, a great burden is added to the func-

tion of that organ. In systole it tugs against the bony chest, every beat being a strain upon its musculature; but the more evident the sign of tugging, the better the condition of the heart-muscle, and the more surely will relief come to it if the overlying bony wall is removed, changing it "into a movable elastic wall formed solely by musculocutaneous flaps." Internal adhesions of the pericardium to the heart may not, of themselves, be of great moment, and usually are difficult to diagnose, but if, with their occurrence, the pericardium itself becomes adherent to neighboring organs, as the mediastinum, diaphragm, sternochondrocostal wall, the function of the heart will be interfered with in degree comparable with the elasticity of these adhesions. The chest-wall itself, except in the intercostal spaces, yields scarcely any at all, consequently the heart-muscle gradually becomes weaker and weaker as a result of the strain. It is in this latter condition, before the heart-muscle itself is worn out, as evidenced by damming-back symptoms, that Brauer proposes the operation cardiolysis, or, as I call it, "Boning of the Precardial Chest-Wall." If threatened heart insufficiency is not too marked, if the liver is not too large, and ascites too great, much relief may be expected; but, above all, the patient must be one whose heart still responds to medication and relief from all strain, which in itself is proof that there is still tone in the heart-muscle, and that it will be markedly benefited by untethering its fastenings to the chondrocostal chest-wall. Hearts weaker than this will scarcely be benefited by operation and will tend to bring any operative procedure into disrepute. Anatomically and, to a lesser extent, clinically, pericarditis may be divided into the following types: diaphragmatic, pleural, chondrocostal, and mediastinopericarditis. The cases may be said almost never to be of the pure type. The existence of the chondro-

costal type is necessary if we are to expect anything from operative interference. The heart is hitched fore and aft to the chest-wall and to the vertebral column by involvement of pleura, pericardium, and posterior mediastinum. It is at this double pull that our therapy is directed. Another indication for removal of the precordial chest-wall is brought forward by Morrison. In a patient with cor bovinum from mitral and aortic disease, who suffered from frequent and severe "angionoid" pains that resisted treatment, he removed four and one-half inches and five and one-half inches of the 5th and 6th ribs, respectively. The result was subjectively and objectively gratifying, so that the patient was able to earn his living, which he had previously been unable to do. He was reported much improved one and one-half years after the operation. Morrison's reasoning was as follows: "Experiments show that direct muscle-stimulation of the eviscerated heart induces contraction. The constant thumping of the heart against the stiff and confining chest-wall gives rise to a cardiac erythism. Morrison believes that one of the causes of pain and increased irritability in greatly hypertrophied hearts is this impact of the forcefully beating heart on the chest-wall. The substitution of a soft tissue-covering for the heart, instead of a bony wall, admits of free action of the hypertrophied heart without the constant hammering of the chest-wall; in other words, the heart has more room in which to work. The term *cardiac decompression* is suggested in the place of *thoracostomy* as used by Morrison, because it better describes the purport of the operation." (Dunn & Summers.)

Essentials for Diagnosis.—The cardinal diagnostic signs of adhesive pericarditis are systolic retraction at the apex, the diastolic shock, epigastric-diaphragmatic tugging, Broadbent's sign, and cardiac and pulmonary immobility. It must be differentiated from Pick's

pseudocirrhosis of the liver with its insidious onset, rapidly recurring ascites after puncture, and intermission and remission with signs of involvement of the pericardium. In our case the sphygmogram, cardiogram, and phlebogram tracings, before and after operation, are remarkable as placing in written evidence the justifiability and benefits of the operative procedure.

History of Case.—J. F., aged 29, consulted Dr. Dunn, Feb. 5, 1912. He is single and is a florist by occupation. Formerly he was a steady drinker, but during the last two years he has drank almost none. It is to be noted that the *only illness he has suffered during his entire life was diphtheria followed by "paralysis" at eleven years of age.*

The family history is negative.

The present illness began gradually, three years before, with "bloating," with tiring easily, and with shortness of breath on exertion. His condition grew rapidly worse, and almost from the beginning he had been entirely incapacitated. He said he could not lie down without several pillows under his head, and that he spent his nights, largely, sitting in a chair; he could not move about without choking and coughing; he was able to eat but little, and his bowels would not move without medicine; his urine was scanty and highly colored; he had had much yellowish sputum in which there were occasionally considerable amounts of blood.

The patient was a well-developed young man. Dyspnea and orthopnea were marked. His face was a dusky red, the veins of his neck and face were turgid, and a marked venous diastolic collapse was present. In the precordium a violent pulsation was to be seen, which consisted of a distinct, rather slow systolic retraction with a rapid vibratory diastolic

thrust of the left costal arch. Broadbent's sign was present. The respiration was rapid and upper thoracic in type. A marked diastolic shock could be palpated. The apex lay in the 6th interspace in the anterior axillary line. Cardiac dullness extended 3 cm. to the right of the sternum and to the left as far as the anterior axillary line in the 6th interspace. The area of heart dullness did not change with change of position. Auscultation revealed a loud systolic murmur heard over the entire precordium. This murmur was best heard at the apex. All of the sounds were blurred; the heart-action was rapid (120 per minute) and irregular. A second diastolic heart-sound could be heard over the entire right ventricle. There was an obliteration of the left lower pleural cavity. The lower border of the left lung in the precordium did not move with respiration. The right pleural cavity contained a moderate amount of fluid.

The abdomen was much distended. The liver extended a hand's breadth below the costal arch in the nipple line; it was tender and pulsated; ascites was present; the scrotum, thighs, legs, and feet were moderately edematous.

The urine was highly colored and sharply acid; the sp. gr., 1,030. Albumin and the aldehyde reaction were present; sugar was absent. Hyaline and granular casts and red blood-cells were quite numerous.

The patient was placed in the Clarkson Hospital, where in the course of three weeks compensation was restored; the heart-action became regular; the murmur entirely disappeared, and the heart-sounds became clear. The patient's temperature was normal during the entire time.

Ten days later, March 13, 1912, cardiolysis was performed. The purpose of the operation was not only to untether the heart by doing away with the

costopericardial adhesions, but also to give the enlarged organ room for freer play. A U-shaped incision commencing four inches from the sternum was made, with its base directed towards the left shoulder, the upper limb skirting the lower border of the second rib, as far as the sternum, thence the curved portion overlapping the left border of the sternum to the cartilage of the sixth rib, the lower limb continuing downwards, outwards, and then upwards, to the anterior axillary line. A musculocutaneous flap down to the ribs was raised, and the third, fourth, fifth, and sixth ribs and their cartilages, covered by their periosteum and perichondrium, were resected flush with the sternum. This exposed the pericardium and left pleura in an area measuring from above down, five inches; from within out, four and one-half inches.

The pericardium was found adherent to the chest-wall and the pleura somewhat thickened and adherent. It was not necessary to ligate the internal mammary artery, as the costal cartilages were first divided externally to the course of this vessel and then the inner stumps were removed. The wound was closed with a cigarette-drain in its lower inner angle. This drain was taken out at the end of twenty-four hours. The operation was followed by a mild bronchopneumonia from which the patient's compensation was little disturbed. In other respects convalescence was uneventful, and the man is today free from all of the distressing symptoms for the relief of which the operation was done.

There seems to be a difference of opinion as to whether or not the periosteum should be removed entirely. In my judgment it should, and in certain cases part of the sternum should be removed likewise. This latter has been done by several operators. The physician who refers the case should be present at the oper-

ation and assist the operator in deciding the amount of bone and cartilage to be removed.

Small openings into the pleura are not dangerous. They can be closed temporarily by gauze pressure and later by suture. In very serious cases where it is expected that a large amount of the chest-wall is to be removed, intratracheal insufflation anesthesia would lessen the operative risk.

As far as I am informed, there have been thirty-one instances of cardiolysis including two unpublished cases of Lejars and my own case. An operation was suggested by M. E. Weill, of Lyons, in 1895, with the object of breaking up adhesions between the pericardium and the heart, and another by Delorme, in 1898—a question of intervention of cardiac adhesions or mediastinopericarditis, the proposition in this latter being exposure of the pericardium, opening it, and freeing the heart and its adhesions by breaking them up or sectioning them, a procedure comparable to pulmonary decortication. Neither of these has been performed, nor is there anything logical in them, since nothing would prevent the reformation of the divided adhesions. In a paper by Roux and Berger, published in *La Semaine Medicale,* Sept. 7, 1910, entitled "Le Traitement Chirurgical de la Mediastino-Pericardite Adhesive," a full discussion of Brauer's operation and of the results obtained to date may be found. They say: "In certain cases the effect has been frankly bad or nothing at all. Nevertheless in the greater number of cases the success of the operation is acknowledged." They insist upon the fact that a long observation after the time of the intervention is essential for an opinion as to the value of the procedure. This is true, and it is not, because in many of these cases the relatively hopeless outlook of a constantly strained heart-musculature leads only to a fatality, whereas, if the strain is relieved, life is prolonged although the original lesion

must of itself, all things being equal, shorten life. In twenty-one cases clearly favorable results have been observed. Amelioration was even noticeable during the operation in some of the cases; the beating of the heart was stronger and more regular; respiration was calmer and narcosis easier; immediately after operation the asystolic condition disappeared, also the dyspnea, edema, etc. However, in other operations the amelioration was more tardy, and in certain ones the condition was even aggravated after the intervention, pulse weak, more marked dyspnea with cyanosis; but these disquieting phenomena disappeared, and little by little a progressive amelioration took place; the palpitations ceased; the edema, ascites, and enlargement of the liver diminished and sometimes disappeared entirely; and a number of these patients who had been condemned to inactivity, were soon able to take up their occupations.

"Examples.—A patient, aged 18, operated on by von Beck, was in perfect health two years after the operation, and was able to do gymnasium work. In Umber's patient, operated on by Fritz Konig, the relief continued for two years and a half. Danielsen's patient, twenty years of age, a butcher, seen again nine months after the operation, remained in an improved condition, considered himself well, and was able to take up his work again. Seen again *six months* after intervention, a case operated on by Kuttner, after having been in a desperate condition, had no pain and had been able to resume work. Another patient of Kuttner's, aged 21 years, was enjoying life five months after the operation. In one of Schlayer's patients, the relief continued at the end of six months. The most favorable cases, and the only ones in which one can speak of the *real, lasting* relief, are those in which the patient is kept under observation for a fairly long time after the operation. In all the other cases there is no

question that amelioration, often very remarkable, takes place immediately after the operation or during observations for only a few weeks. These results show appreciable relief and deserve to be taken into consideration."

There are seven cases of failure, and their study is important because they serve as indices in the establishment of the exact indications and contra-indications for operation. Of these, one died twelve hours after operation, from a progressive weakening of the heart (Schlayer's case); in another (Poynton & Trotter), there was a temporary improvement, but the symptoms reappeared with a fatal termination.

Of the five others one was not relieved by operation, but the case was complicated by mitral insufficiency. In another the indications for operation were not present and the autopsy showed simply adhesion of the two leaflets of the pericardium, without other complicating adhesions; besides, the patient was submitted to a series of abdominal operations, as decapsulation of the right kidney, splenopexie, hepatopexie, and omentopexie, all of which was a parody upon surgery, at best.

In the first of the two cases operated upon by Lejars, the symptoms insisted upon by Brauer, to make the diagnosis and the index for operation, were not positive; no attempt at preliminary medical treatment, with rest in bed, was made; the patient died one month after operation. The same criticism may be made as to Lejars' second case, and the operative procedure showed "very small elements in favor of adherent mediastinopericarditis. All of the symptoms persisted and two months after the operation the patient succumbed." It would seem that Lejars had not studied carefully the kind of cases in which cardiolysis is a hopeful operation.

Roux-Berger concludes: "If Brauer's operation is used only in cases which are clearly justifiable, in veritable mediastinopericarditis adhesions one will be right in expecting very good results, i. e., immediate great relief, and, in truth, in a great number of cases, a permanently successful outcome."

To those who are sufficiently interested I would suggest the study of papers given in the subjoined bibliography, as they, with their references, contain, as far as I am informed, all that has been written upon the subject. I have found nothing in American literature.

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DISCUSSION

Dr. James E. Moore (Minneapolis, Minn.): I was requested by Dr. Summers to discuss his paper, but as I was attending a meeting of the Council I did not hear it. There is practically nothing to discuss from my point of view, because I have never done the operation. I have read the literature. He has summed it up very nicely. It sounds like a sensible procedure, and the experience of those who have performed the operation has demonstrated that it is free from danger. These patients are terrible sufferers. I had one case that presented itself to me in which operation had been suggested; but this woman had an enlarged liver, and I did not dare to operate. Her condition was too far advanced. If I had seen her earlier and operated, her life might have been prolonged. I believe it is a sane, sensible operation.

Dr. Summers (closing the discussion): So far as removing the periosteum is concerned, I believe that it is the only thing to do, because in some cases operated upon there has been reproduction of the removed bone of the ribs, and reformation of the chest wall, with retraction of the same, and this has defeated the object of the operation. At any rate, the operation as

performed by me gave wonderful relief to the patient. The great trouble with us as surgeons is that too few are good general diagnosticians; we are all thinking so much of surgery. I think it would be wise for us to occasionally take a post-graduate course in internal medicine, so that we might diagnose these and other medicosurgical cases accurately and not use bad judgment in selecting patients for operation.

FRACTURE OF THE CARPAL SCAPHOID

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Since the introduction of the *x*-ray as a diagnostic agent a considerable number of cases of fracture, as well as dislocation, of the small bones of the wrist and of the ankle have been reported, and several very complete contributions upon these subjects have appeared in the surgical literature during the last ten years. It is not our purpose therefore to add anything to the already very complete and accurate descriptions of this condition that have already been given, but rather, to freshen the memory of a subject perhaps already familiar to all.

It appears that we are all prone to treat too lightly some of these so-called minor injuries, even when the facts and possibilities are quite well understood, and it has seemed to the writer more than likely that some of the cases ordinarily passing as "sprained wrists" might, on more careful study, be found to belong to that class of injuries indicated by the title of this paper.

It is with this possibility in mind that a brief recital of a case recently coming under observation is ventured.

C. E. M., aged 26, male, physical director in a gymnasium, presented himself June 29, 1911, complaining of pain in the left wrist on sharp flexion, but no pain on straight pulling. He stated that four years previously, while playing basket-ball, he was knocked off his feet, falling on the over-extended hands. Since

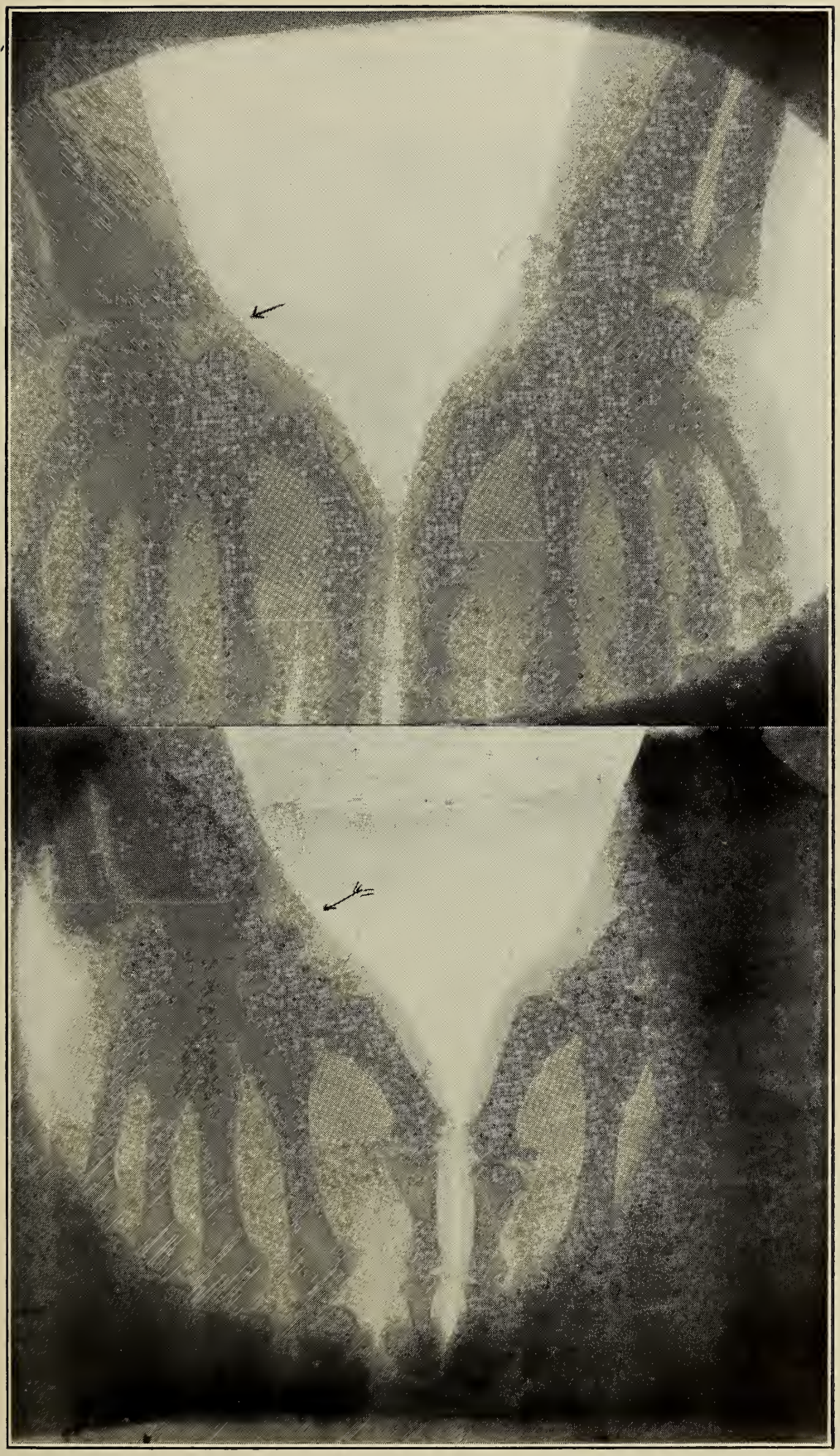
that time he had noticed the wrist was weaker than formerly, certain motions causing the wrist to give way, and a small, hard nodule could be seen on the wrist below the end of the radius. This nodule stood out more prominently when the wrist was bent to the ulnar side, and pressure on this small swelling was intensely painful. Following the injury the wrist was bandaged, but not immobilized, the condition being regarded as a severe sprain of the wrist.

Four weeks previous to the time he presented himself he had sustained a second fall upon the extended hand during a game of tennis. Since the second fall the condition had been much worse, the wrist being so painful as to prevent his engaging in bar and other exercises throwing sudden, severe strain upon the wrist.

On examination a hard, round, slightly movable mass could be felt on the radial side of the wrist, and pressure on this mass caused sharp pain. Over-extension of the wrist was distinctly painful, and bending the hand toward the radial side produced a feeling the patient described as something "slipping" in the wrist.

An *x*-ray plate was made, taking the normal and affected wrists on the same plate. The plate showed a very definite fracture through the neck of the scaphoid with outward rotation of the upper fragment and some irregularity along the border of the fractured surface.

The patient was advised to have an excision of the proximal fragment, to which he consented. Under ether anesthesia, a longitudinal incision was made upon the dorsum of the wrist, just to the inner side of the tendon of the extensor carpi radialis longior, making the incision through the skin and annular ligament, as advised by Codman and Chase. The two fragments were found to be freely movable upon each



Upper Fig. Fracture of the scaphoid through the middle.

Lower Fig. Present appearance one and one-half years after the removal of the proximal segment.

other and ivory-like in appearance with smoothed surfaces approximating. The proximal fragment was pulled up, freed from its attachments, and removed. A small gutta-percha drain was inserted, the edges of the annular ligament brought together with catgut, and the skin approximated with horsehair. There was a small amount of synovial discharge for two or three days, and after the removal of the drain the wound closed by primary union.

On examining the patient one and one-half years after operation no tenderness was to be made out, and the patient says there is no pain on motion, but that the wrist is a little weaker than the other. He is at present engaged in his work as physical director and is able to engage in most of the exercises pertaining to his work without pain or marked discomfort.

This case presents the typical history usually presented by these cases, including the treatment for sprain with its unsatisfactory result, and serves to emphasize again the fact that all cases of injury of the wrist presenting any considerable pain or disability should have a skiagraph of both wrists taken on the same plate, in order that any deformity may not be overlooked. It has been emphasized by Downes, who has reported a series of seven cases from the out-patient department of Bellevue and New York Hospitals, and by Codman and Chase, who have reported eighteen cases, that early diagnosis and early and long immobilization furnish the only possibility of securing union. Downes reported seven cases observed from January, 1907, to January, 1908, and adds that in seven years out-patient service he had not recognized a single case. This experience corresponds with that of Codman and Chase, and serves to emphasize the value of the *x*-ray as a diagnostic agent in these cases.

The symptoms presented in cases of fracture of the

scaphoid depend somewhat upon the time that has elapsed after receiving the injury. The patient generally gives a history of a fall upon the extended hand in the same manner as those who have sustained a Colles fracture of the radius. This is followed by pain and swelling and disability. The pain in most cases is quite severe and the swelling is on the dorsal aspect and radial side of the wrist. Extension is limited to a very few degrees, while flexion is less interfered with. On examination the ulnar and radial styloids are found in their normal relations, and pressure over the scaphoid with the hand flexed causes intense pain. Ecchymosis is rarely, if ever, present. Downes found crepitus in two of the seven cases reported by him, while Codman and Chase did not find crepitus in any of their eighteen cases. The cases reported by Codman and Chase were practically all late cases; that is, cases in which a period of several weeks to several years had elapsed since the injury. The absence of ecchymosis may be explained by the fact that the blood-supply of the carpal bones is very limited, and the small amount of blood escaping is contained in the synovial capsule of the joint and prevented from escaping betweeen the ligaments and tendons into the subcutaneous tissues. The relative infrequence of crepitus may also be explained by the fact that the carpal bones fit very closely and, even when fractured, allow but a very limited amount of motion upon each other.

Simple sprain, as a rule, recovers promptly, and the continuance of pain or disability for any considerable period should arouse strong suspicions of the presence of a fracture of the carpal scaphoid or fracture of the scaphoid with dislocation of the semilunar bone.

Codman and Chase have emphasized the value of the *x*-ray in making a definite diagnosis, and have pointed out the necessity of skiagraphing both wrists

at the same time for comparison. It has been shown that the carpal scaphoid develops from two separate centers of ossification, and hence there have been found a few cases of so-called bipartite scaphoid. This condition, though rare, might possibly lead to a misinterpretation of the *x*-ray plate if but one wrist were included in the skiagraph. The anomalous condition of bipartite scaphoid, when present, is generally present in both wrists, and hence the finding of a separation of the bone *on one side only* would speak for a traumatic as against a developmental origin.

The line of fracture is often quite irregular or may run in places at an angle with the long axis of the arm, hence, in cases in which there is any irregularity of outline in the suspected scaphoid, several plates should be taken at different angles or positions so as to secure, if possible, a plate in which the rays pass in the places of fracture.

In Downes' series of seven cases the fracture was through the middle third in six cases, and at the junction of the middle and lower third in one case. The styloid process of the radius impinging upon the middle of the scaphoid at a point about its middle third when the hand is forcibly extended and turned toward the radial side, may explain the large preponderance of fractures through the middle of the scaphoid.

The treatment to be carried out depends upon whether the case is seen early or late. Seen and diagnosed early, the more conservative treatment by means of a molded-plaster splint applied on the palmar surface, extending from the base of the fingers to the elbow, with an adhesive strap about the wrist at the site of the fracture, and the hand held straight, should be tried, especially in cases in which the bone is not comminuted, or in which the fragments are not badly dislocated. Authorities differ as to the time of im-

mobilization, but all agree that the period should be long,—from three to six weeks.

In cases with comminution or dislocation of fragments, operative measures should be resorted to at once, and one or all the fragments removed, depending upon the degree of comminution or dislocation.

In cases seen after several weeks, months, or years have elapsed, experience has shown that attempts to secure union by such means as drilling, wiring, etc., have been uniformly unsuccessful. The treatment to be employed in these cases is the operative removal of one or both fragments, the consensus of opinion being in favor of removal of the proximal fragment, as was done in our case.

The dividing line between early and late cases seems somewhat difficult to determine. Union has apparently taken place in cases in which the diagnosis has been made several weeks after the injury by immobilization for three or four weeks, followed by massage and passive motion, with bandaging for another period of three or four weeks. On the other hand, in some of the cases in which union has apparently taken place when handled by the conservative method, the pain and disability have persisted, due perhaps to secondary arthritis and joint-changes following the injury. Codman and Chase summarize their exhaustive papers by the following conclusions:

1. Cases which have been treated as sprains by a short period of fixation, followed by massage, active and passive motion, seldom, if ever, have union of fragments.

2. If the joint is kept fixed for a number of weeks following the injury, union may occur, but the functional result is not perfect, although better than in cases of non-union.

3. It is too late to obtain union if fixation is not attempted until a few weeks after the injury.

4. Excision of the proximal half of the broken bone promises a better ultimate result than any other form of treatment.

5. Since operation is an undesirable risk, a reasonable attempt should be made to secure union by fixation, if the case is seen soon after the injury.

6. Operation should not be delayed many months, because secondary joint-changes may occur and chronic arthritis result.

7. The advisability of operation in cases of long standing is doubtful, and must be decided by the amount of disability present in each individual case.

FOR DISCUSSION SEE PAGE 292

A METHOD OF BONE-PLATING

Carl E. Black, A. M., M. D.

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A study of the work of Mr. Lane and his followers and comparing it with that of Lambotte[1] and his followers will convince one that the last word has not been said on retaining the fragments in the operative treatment of fractures.

While I cannot claim an extended experience the cases I have operated on have impressed me with two difficulties: first, the necessity of providing, at all times, as full an assortment of plates, many of which will be rarely called for or will not be adapted to the given case; second, the ordinary plates have too much bulk and are too heavy.

In support of the latter statement I quote the following sentence from Mr. Lane[2]: "The chief obstacle that has to be overcome in many cases is the approximation of the skin-incision and its retention in accurate apposition."

I venture to present a plan of bone-plating which, so far as I know, is new. I have not yet had sufficient experience to present conclusions, but I put it before you as a suggestion for trial.

In dealing with fractures the object to be attained is correct apposition. This includes several important points. The normal length and correct contour must be maintained. Fragments may have to be held in position and the companion bone or the neighboring joint may have to be dealt with. It may be fairly as-

1. L'Intervention Operatoire dans les Fractures.
2. The Lancet: (London) November 4, 1911, No. 4601, vol. clxxxi.

sumed that no one plan is equally well adapted to all cases and that any plan may have to be modified to meet the requirements of each case. Therefore, other things being equal, the plan which is susceptible of the greatest modification at the operating-table should give the best results.

Proper tools have not yet been developed. (Fig. 1.) Speaking of clamps, most of those on the market are altogether too heavy and cumbersome for rapid and accurate work. Awkward and unwieldy instruments interfere with asepsis. We need to study the clock-maker, the locksmith, and the cabinet-maker and their equipments. (Fig. 2.) Either might make a better "bone surgeon" than the average general surgeon had he the aseptic technic. Unfortunately for the mechanics of the work, asepsis is of first importance.

I cannot subscribe to the dictum of Lane that[3] "in the vast majority of cases I prefer to use as long and as strong a steel plate as possible, carrying as many screws as space permits."

The average plate sold in the shops is altogether too bulky, being made to depend on its bending or breaking strength for success. After a certain point, the strength of the plate is only what the screw will hold in the bone. I believe this is to be a wrong principle in plating. Nature did not fashion the bones on this plan. They are a system of supporting levers with reinforcement at the points of strain. The plating of a fracture is only a temporary device and should have the least bulk consistent with providing the support and meeting the strain required during the period of healing.

The plan I suggest might be called "contour"-plating in distinction to direct plating, which relies on the breaking-strength of the plate. If the plate is applied in the direction of the strain by which the fragments

3. The Lancet (London), June 12, 1909.

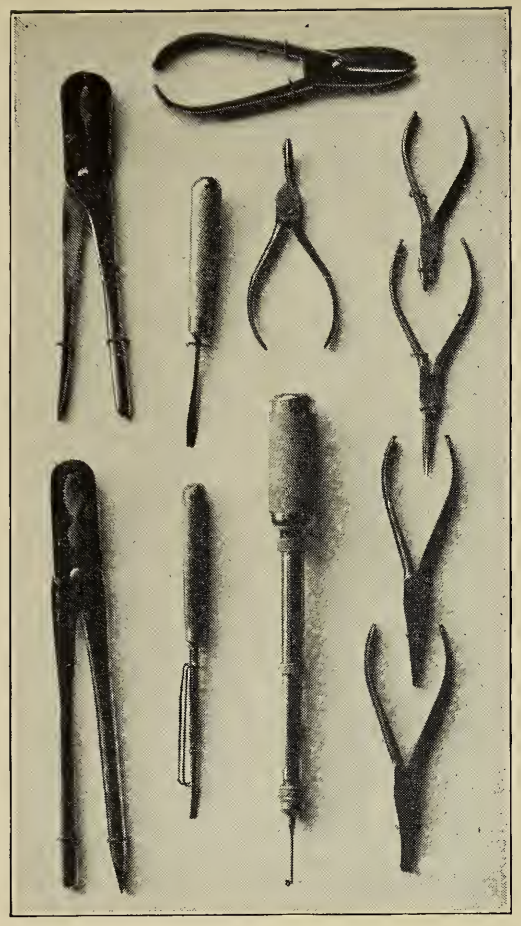

Fig. 2. Tools purchased in the hardware store and used in experimental bone-plating and also in a number of cases at the operating-table.

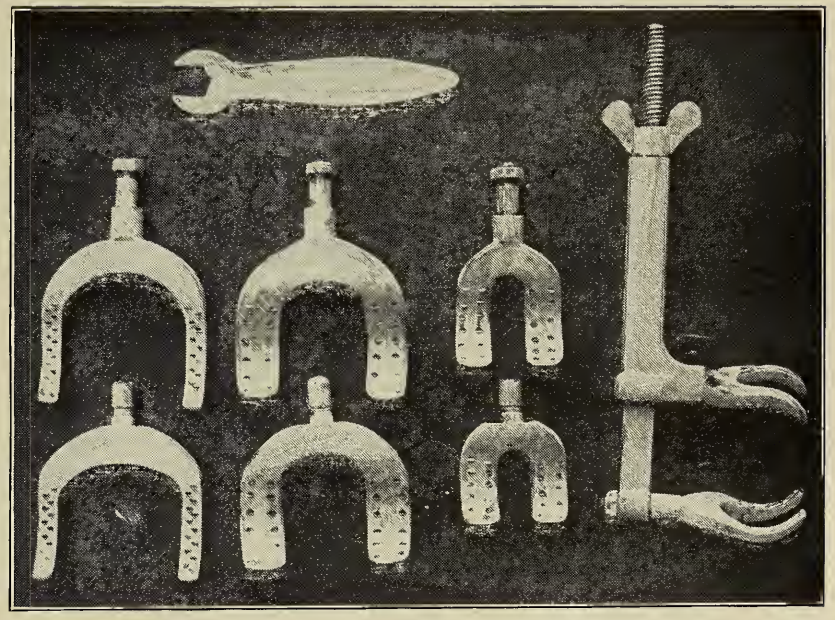

Fig. 1. Clamp for holding bone fragments in position while plate is applied. It is purposely designed not to hold the plate under the clamp.

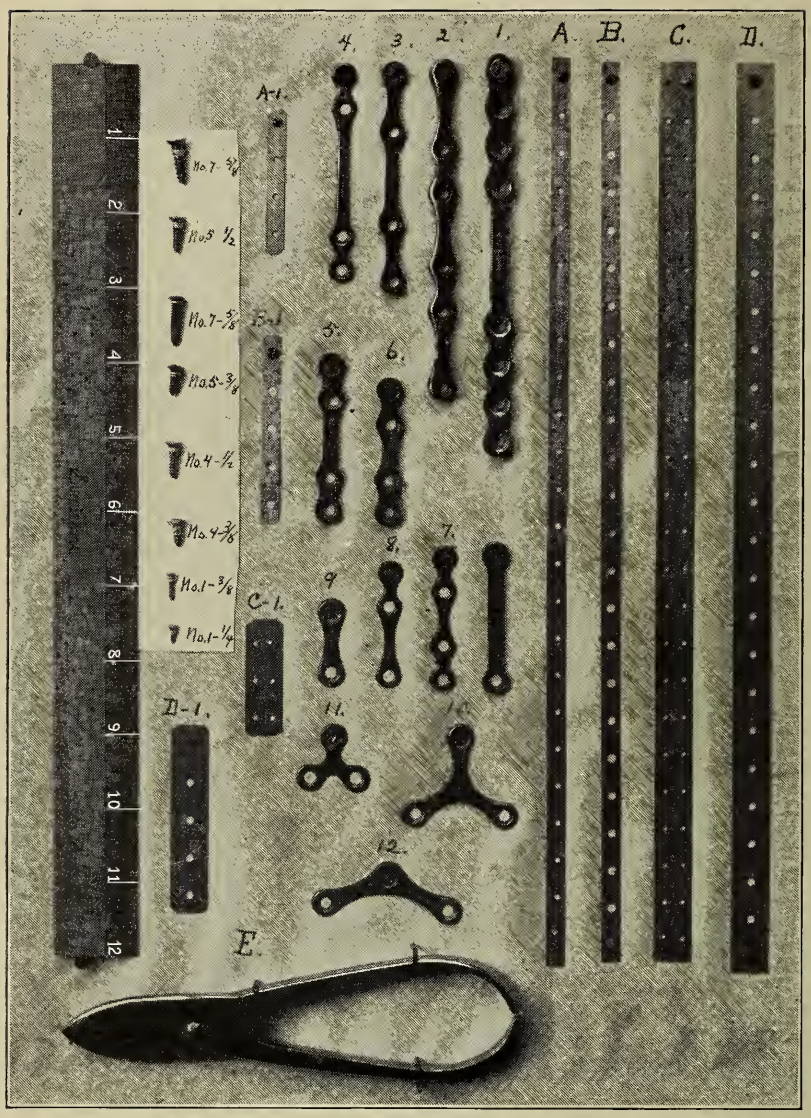

Fig. 3. Lane's plates and screws of various sizes and steel ribbon proposed. Nos. 1 to 10, Lane's plates in various sizes. No. 5 to No. 7, screws used for Lane's plates. No. 1 and No. 4, screws in two sizes, ¼-inch and ⅜-inch. A, B, C, D, steel ribbon in two sizes punched for No. 1 and No. 4 screws. A and B are ¼-inch wide and 1/128-inch thick; C and D are ⅜-inch wide and 1/64-inch thick. A-1, B-1, C-1, D-1, plates made at operating-table from steel ribbon. E shears for cutting steel ribbon.

are likely to be displaced a much lighter plate and screw can be used, because the pull is applied to the screws in the direction which gives them the greatest holding-strength.

In trying to carry out this principle I have used untempered clock-spring steel of various thicknesses and widths. Tentatively, I have adopted two sizes. The first is a steel ribbon ¼ inch wide, $\frac{1}{128}$ inch thick, and 12 inches long with holes for No. 1 screws placed ½ inch from center to center. It is important to have all holes exactly the same distance apart so that, in special cases, several thicknesses of plate may be used. To this end some of the ¼-inch ribbons have been punched with No. 4 holes, and in order to put in a large number of small screws where the bone is frail or fragmentary some of the ⅜-inch ribbon is punched with a double row of No. 1 holes. From a recent experience I am convinced that in order to meet varying conditions the holes should be ¼ inch apart instead of ½ inch. (Fig. 3.)

The advantages which suggest themselves for this method are that plates may be adapted at the operating-table to accurately fit any contour, and that these may be made as long or as thick as required. Usually, the narrowest and thinnest plates placed on either side of the bone and in a direction to meet the greatest strain which will be applied, will meet every indication. In an experimental way I have tried to illustrate a few fractures retained by these thin plates. (Fig. 4.) If one can judge by the force which these bones will withstand, the thin plate would seem to be all-sufficient. The real strength is in the screws (Fig. 5) and by placing one plate opposite to another there is a pull put upon the plate instead of a bending strain.

In other words, we follow the principle of the lever and can secure from one or two narrow thin steel ribbons more strength than from twenty times the weight

of steel in one large plate depending on its bending or breaking strength.

The plan is especially adapted to torsion fractures where the ribbon can be so placed as to overcome the twisting strain.

In this way the screw is enabled to give a maximum of efficiency, or rather we make a small screw and a light plate furnish more strength than a heavy plate and a large screw would furnish under direct strain. The ideal screw is one which carries its own drill-point, such as those used by Lambotte. With a proper chuck for the drill-brace the screws can be placed rapidly and the necessity for drilling the holes separately, finding them afterward, and then putting in the screws will be avoided. (Figs. 6, 7, and 8.)

The smallest plate sold in the market is three to six times as thick as this thin steel ribbon; the heaviest is twenty times as thick, and the screws are in proportion. It is useless to have the screws extend beyond the dense bone, and most of those used in plating are too long. It is rare to find the dense screw-holding bone more than ¼ inch thick, and it will not average more than ⅛ inch.

To summarize: By using these thin steel ribbons the spaces for the accumulation of blood and serum are comparatively small and infection less likely; the periosteum can be more nearly approximated; the remaining foreign body is from three to ten times less; the fascia and skin can be carefully approximated without the least difficulty.

FOR DISCUSSION SEE PAGE 292

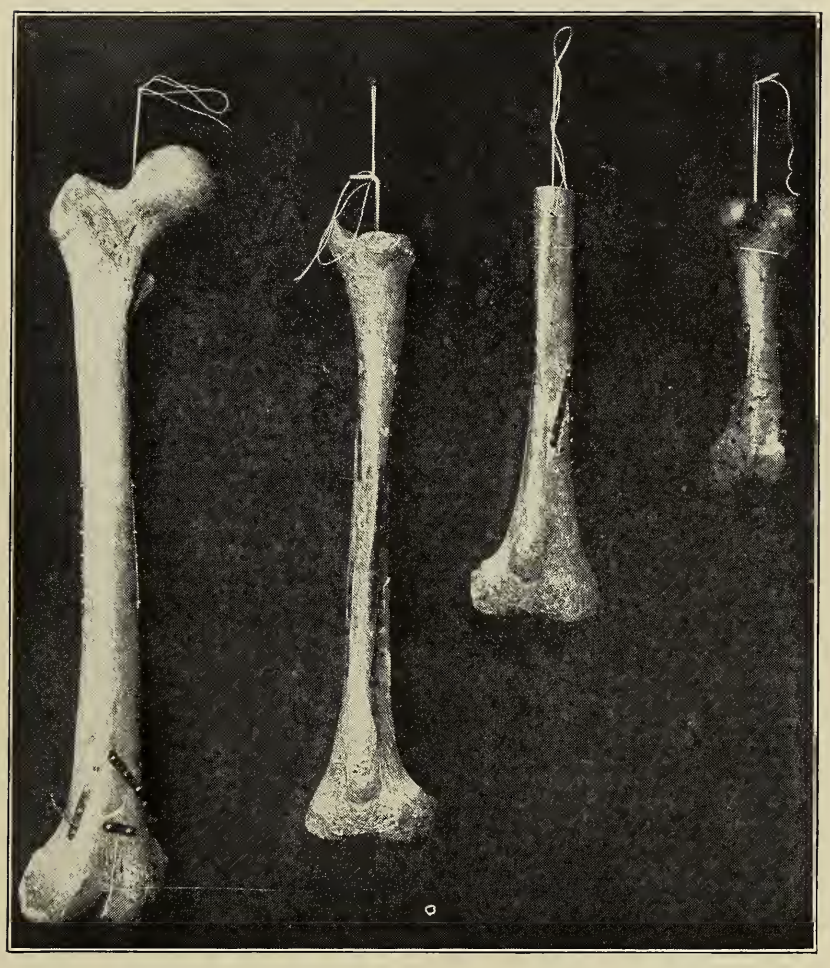

Fig. 4. Front view. Illustrations of experiments in contour bone-plating with steel ribbon ¼-inch wide and $\frac{1}{128}$-inch thick, carrying No. 1 ¼-inch screws.

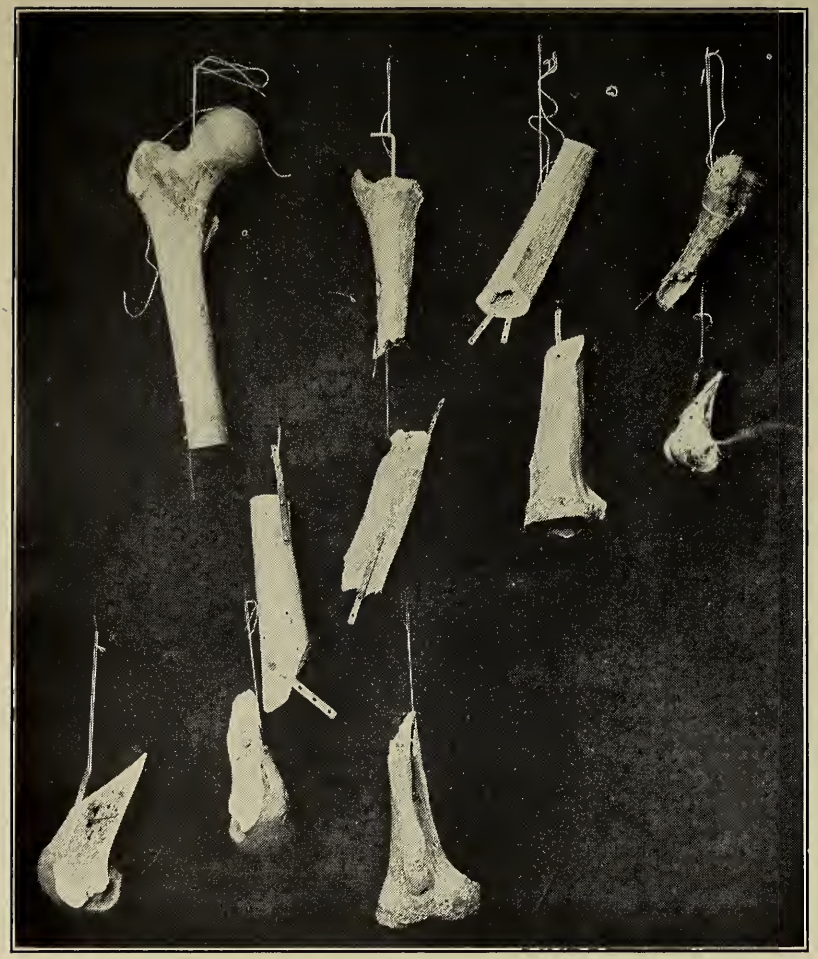

Fig. 5. Fragments separated. Illustrations of experiments in contour bone-plating with steel ribbon ¼-inch wide and 1/128-inch thick, carrying No. 1 ¼-inch screws.

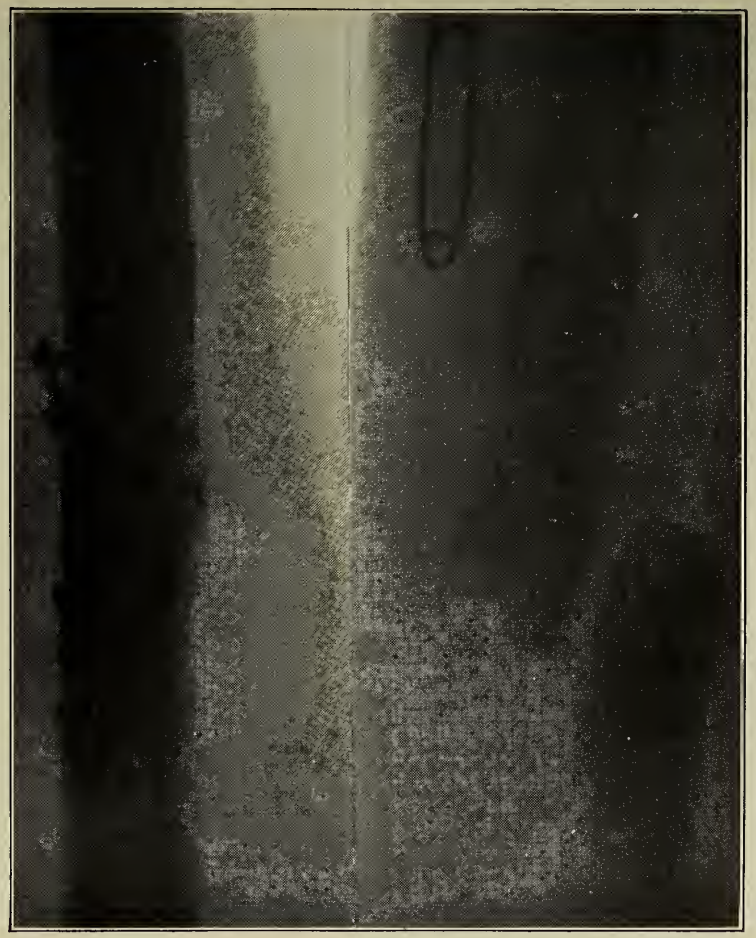

Fig. 6. Fracture of middle of humerus. Man aged 40 years. Retained by two ¼-inch ribbon plates, 1/128-inch thick with No. 1 screws ¼-inch long.

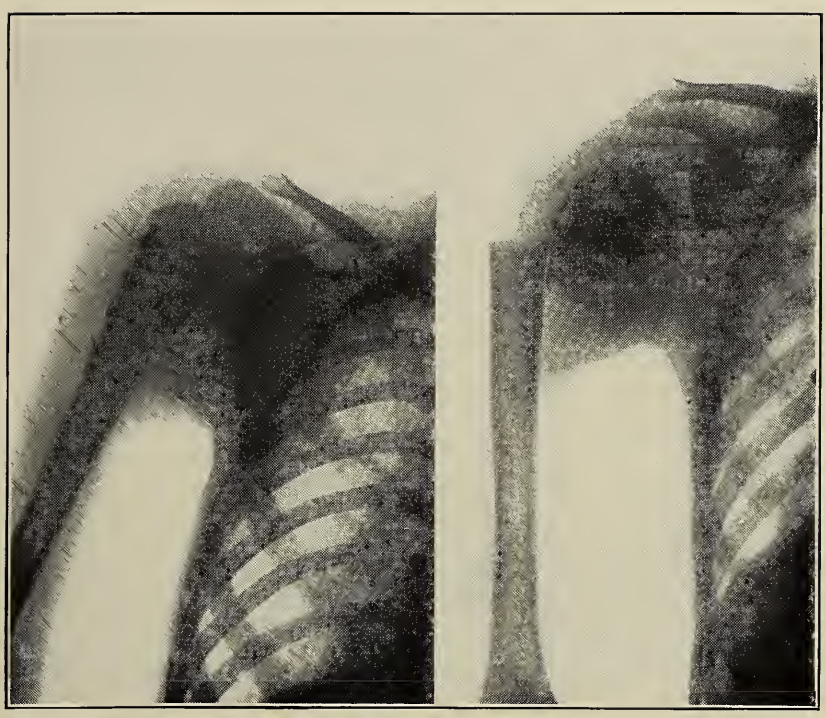

Fig. 7. Fracture of humerus. Boy aged 6 years. Retained by ¼-inch steel ribbon and ¼-inch No. 1 screws. Note length of screws in the bone.

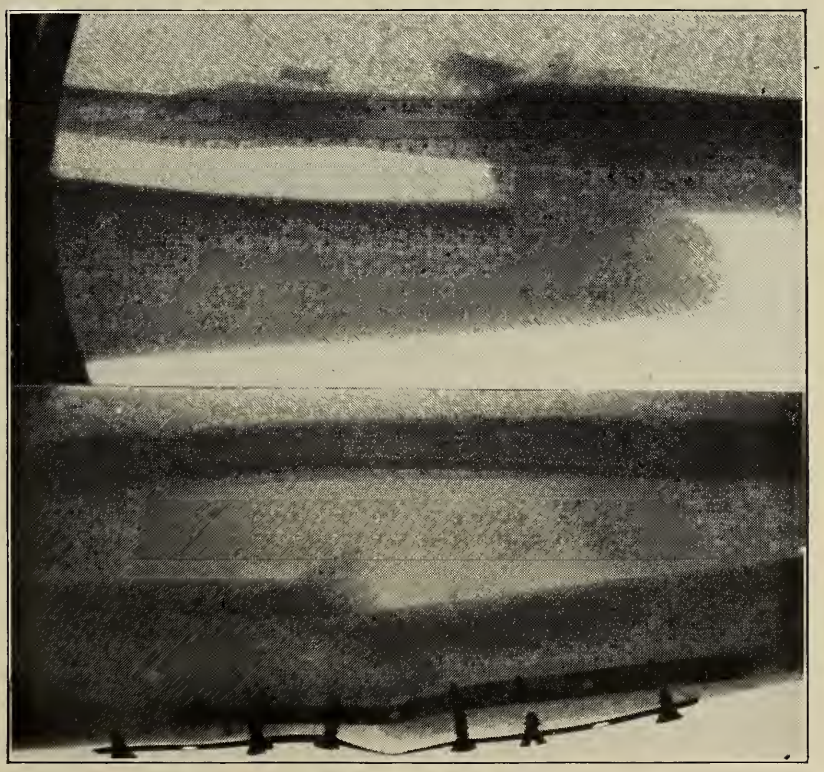

Fig. 8. Compound fracture of tibia. Man aged 64 years. Retained by two ¼-inch steel ribbons and ¼-inch and ⅜-inch No. 4 screws.

SOME PHASES IN THE MODERN TREATMENT OF FRACTURES

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From the viewpoint of the general surgeon there has recently been presented a number of new, betimes startling, innovations in the consideration of the treatment of fractures.

With the general application of the *x*-ray in the diagnosis of these injuries there quickly followed an insistent demand for better anatomical and functional results than had previously been obtained. This very naturally led to more frequent exploration of the site of injury with a view to obtaining a better conception of the character and extent of the damages to the structures overlying the broken bones.

The field for applying retaining apparatus to the fragments rapidly widened until today this field, in some localities, is limited only by the number of fracture cases applying for treatment, and while none may gainsay the immeasurable value of the lesson taught by the *x*-ray, still this very valuable agent has been responsible for much misinformation, a number of suits for malpractice, and many needless operations. A decade ago the ray was considered by many to be infallible, not alone in establishing the diagnosis, but also in determining what the final result would be in a given injury.

We now know how utterly fallacious these claims were, and we are still busily engaged in separating the

wheat from the chaff. The superficial examinations engendered by the use of the *x*-ray and the faulty treatment based upon such misconceptions are slowly being brushed aside, and in lieu thereof we are applying the knowledge gained from the use of the *x*-ray as a link in the chain of clinical evidence, which must ultimately make for accuracy in diagnosis and the intelligent application of knowledge in the management of fractures.

The use of an anesthetic plus experience, natural adaptability on the part of the doctor, or a "mechanical sense," as Cotton would say, are factors of prime importance in the management of fracture cases. Personally I think the real value of the *x*-ray as applied to fractures, lies in its employment after reduction of the deformity has been effected. By the use of the *x*-ray soon after the primary dressing, one may accurately judge of the efficiency of his treatment, and, if the fragments are not in proper relation, proceed to correct any remaining deformity.

I say soon after primary dressing. This does not imply that a ray must be taken on the same day the reduction had been accomplished; the second or third day will answer; and this leads me to speak of a common observation, namely, the average doctor goes to a fracture case very similar to the department responding to an alarm of fire. There is really no necessity for such haste. No doubt many faulty results may be charged to this unseeming haste on the part of the attendant.

Continuance of pain after reduction in fracture cases, is an indication that reduction is incomplete or that some displacement has followed your treatment; in either event re-examination, preferably under full anesthesia, is mandatory.

The use of massage soon after the receipt of an

injury causing fracture of one or more bones, is a very old practice, which is seemingly very much neglected by modern surgeons.

Light massage, beginning at the end of the first week, will increase lymph- and blood-circulation, maintain tone in the limb, and increase reparative powers.

In a paper presented to this Association some ten years ago I called attention to the value of massage and early passive motion in the treatment of fractures; to the fact that fractures were being over-treated by allowing the limb to remain fixed for a month or six weeks in one position; and, furthermore, that such treatment was responsible for an incalculable amount of functional abridgment in the final outcome of fracture cases. This sentiment is reiterated to accentuate the necessity for the use of this valuable adjunct in the successful management of fractures, and to call your attention to a recent valuable contribution on the subject by Mennell and Lucas-Championniere.

I cannot forego this opportunity to remind you that repair of broken bones is not wholly dependent upon careful reduction and absence of soft structures between the ends of the fragments. Poor health, specific disease, the excessive use of alcohol, and tuberculosis are recognized as factors in the production of delayed union or non-union, but occasionally we meet with a patient possessing an inherent something which inhibits bone-forming elements. I have two such cases in my practice, one a strong healthy man of 35 with an ununited fracture of the mid-humerus. He declines operation because he has satisfactory function. In the other case the fragments were fixed and held in good relation for a period of three months, at which time one plate was removed, but no evidence of any attempt at bone-repair could be discovered.

In some instances the force which produces the

fracture continues long after the bone gives way, causing extensive damage to the soft structures, favoring delayed union or non-union. The extent of such damage cannot be determined by an external examination, and no physician should be held responsible for poor end-results in such cases.

Concerning the open method in the treatment of fractures, we may say, in summary, that, given a competent surgeon with good hospital facilities, those fractures which may be reached in which the ends of the broken bone cannot be kept in proper relation to allow union to take place with a reasonable assurance of overcoming marked deformity and the restoration of function, should be subjected to some type of open method in their management; that fractures occurring in the long bones should, after complete reduction, show crepitus as a final test for freedom from interposed soft structures, and, in the absence of this confirmatory evidence, union will depend upon examination through an incision and removal of the interposing structures; that all open fractures should, after proper cleansing, be freely exposed, reduced, and retained by appropriate mechanical means; finally, that ununited or old fractures in which union had taken place at a faulty angle, inducing pain and loss of function, should be subjected to operation.

The great handicap here is the liability to local and general infection. One may be sure of his technic in practically the entire realm of surgical endeavor and still fail miserably in dealing with fracture cases. Although instances are not wanting wherein infection, long morbidity, and death followed operation in uncomplicated fractures, the very nature and gravity of the circumstances which result in the production of cases demanding operation, the class under consideration, are such as will greatly favor germ-invasion.

In addition to fracture of the bone we have damaged lymphatics, blood-vessels, nerves, muscles, and tendons, which serve to diminish local resistance, and a varying degree of shock incident to injury and operation, which serves to break down the general barriers to infection.

The retention in the wound of a more or less cumbersome contrivance fixed to the bone, has no analogue in modern surgery and, while essential to success, surely favors the process of infection.

Finally, we have no friendly ally, such as the peritoneum, to destroy organisms unavoidably introduced, which are present in all operating fields save possibly those produced by the cautery.

Celerity in operating was the chief factor in avoiding mortality in the pre-antiseptic era, and will count for much in the prevention of sepsis in the class of operations under discussion.

The fragments should be exposed by a combined sharp and blunt dissection through an avascular portion of the limb. Nothing but instruments should be introduced into the wound, and these should be rinsed in very hot water each time after using and before reintroducing them into the field of operation.

With the fragments well exposed one may gently lift the broken ends by the aid of forceps until they appose each other, and then, by pressing with some flat instrument over the site of the break, reduce the deformity preparatory to fixing the fragments by some mechanical means, preferably chromicized gut or kangaroo tendon.

Great difficulty is not infrequently experienced in getting the fragments in such position that retaining apparatus may be placed, and this has caused the introduction of numerous devices to assist in the reduc-

tion, but more will depend upon the mechanical sense of the operator than the instruments at his command.

The skin incision is closed with clips, and the walls of the wound permitted to fall together without retaining sutures.

Immobilizing the limb after fixing the fragments is, if any difference, more important than in the treatment of fractures by the closed method.

DISCUSSING THE THREE PRECEDING PAPERS.

Dr. William Hessert (Chicago): With reference to the first paper by Dr. Smith relative to fractures of the carpal scaphoid: My experience has been no greater than that which comes to any general surgeon who sees a fair number of cases of fractures. I have had only four or five cases of fracture of the scaphoid. My first experience with injuries of the wrist of this nature was some ten years ago, when I encountered a case of dislocation of both the scaphoid and semilunar bones in a young person who had sustained this injury in the usual manner by forcible hyper-extension, just as a Colles' fracture is sustained. He came under my observation about six weeks after the injury, and after his case had been treated as one of Colles' fracture, with the dislocated carpal bones appearing on the palmar surface of the wrist and causing considerable disability and pain. These two bones were removed, and one of the points Dr. Smith wanted me to bring out in my discussion was the question of function after removal of a portion of the bone, the whole bone, or two bones. In this case, where two bones were removed, I was able to follow up this man until a few years ago. He was a painter by occupation, and he resumed his work. I saw him at work in my vicinity one day, and evidently function was well restored. There was some limitation of motion, and the function, I dare say, was 85 per cent restoration, possibly more, so that I fully concur in the removal of a portion of the scaphoid if it has been fractured at the neck, where it usually is, in cases seen late, and in which there is pain and disability following improper treatment. The removal of a portion or the entire bone is perfectly justifiable, and I think the results will be much better than if a fragment is left.

The question of diagnosis is important, because many cases slip through under the diagnosis of sprain, and in those cases

closely observed, where they go to a surgeon under a diagnosis of sprain, the *x*-ray will clear up many of them. We have had cases in which an early diagnosis was made and the bones not displaced, and we have retained them for a long time, say six weeks or more, in a plaster cast, the result being excellent, so that, if the cases are recognized early and treated properly, there will be no need of any later exsection.

In regard to Dr. Black's paper on bone plating: I quite agree with the proposition to put as little wire in a fracture, where we operate for simple fractures, as possible. We ought to go farther than that. I think the tendency ought to be now for the pendulum to swing back again. There has been too much operating on fresh fractures; we have gotten away from some of the methods of the older surgeons in treating fractures, and we ought to limit the indications for operation. Where we have to operate, after we have effected reduction and retained the fragments without putting in any foreign body, we ought to observe the most absolute asepsis. When it comes to the question of plating, screwing, or nailing, I do not think it makes much difference just what sort of plate is used so long as you operate rapidly, with the greatest dexterity, and observe all the details and lines of technic. It seems to me that it will make little or no difference whether you use a Lane plate or a small steel plate. It would appear to me that the technic required in the plating of bones should be looked at from two or three different aspects, namely, the amount of manipulation necessary, the danger of necrosis, and the danger of infection in a small percentage of cases.

Dr. James F. Percy (Galesburg, Ill.): This paper of Dr. Haines is merely the beginning of a wave that is going to sweep over the country, protesting against the so-called Lane operation, and the reason for it is that the operation is being abused. It is going to have the same relationship to bone-surgery that ovariotomy in the early day had to gynecology. Unfortunately, like the ovariotomies in the early days, there were no reports of those who died. There are practically no reports being made of the work done by the average man who infects these cases. There are a lot of these developing. I hear of them every once in a while, and I have been brought in contact personally with a number of them, and I am sure this must be true in the experience of every man who does very much consultation work. It is perhaps well here to illustrate this phase of this talk by a case that came under my observation recently.

A young man, twenty-four years of age, fell off of a haystack and broke his thigh at the junction of the upper and

middle thirds. He was kept in bed for thirteen weeks with weights merely on the leg, and then they discovered there was no union. Then he was sent to a so-called general surgeon, a man who does a lot of work, and he put on a Lane plate in a farm-house. To make a long story short, I took off this boy's thigh just a few days ago. He was reduced from 180 pounds to 90, and I never saw so much suppuration in a case in my life.

This case brings out another fact which it is well to call attention to, and which is emphasized in Dr. Black's paper; *i. e.*, the point of infection. So far as the osteomyelitis was concerned in this case, it was through the screw-holes of the Lane screws. The *x*-ray showed two beautiful openings, one above and one below, where the pus was coming out, with an entire destruction of the medullary canal. I will say, in passing, that I never saw so much pus exude from a human being as in this case. The parents of the boy had learned to make him wear paper pans. These were six inches long and four inches wide and an inch deep. He would fill one of these pans in ten minutes with pus from the sinuses that developed in the thigh. That was going on night and day. I took off the leg because I was afraid of amyloid changes that might occur in his kidneys. It is a curious thing, or rather an interesting thing, that you should get an infection without reprehensible carelessness in a late ununited fracture, or a fracture that has been long ununited, because the tissues are so toughened that we ought to get good results with ordinary asepsis on the basis of what Murphy does when he uses his formalin-glycerin injections into the joint, toughening the endothelium. We get the same results in our ununited fractures. We get resistant tissue, and we ought not to have infection; but, going back to my first point, we are having these infections. The time is coming when this Association will condemn the application of any form of foreign body, except by men who are peculiarly qualified to do this work. It is just as dangerous for the average man to attempt the Wertheim operation, and it is something that we cannot emphasize too much.

A word or two more in reference to Dr. Black's paper, and that is as to what he tells us about the work of Lambotte a long time ago. Dr. Lambotte did all this work now known as Lane's work, and Lambotte has a drill on the end of the screw, so that he does not use the ordinary drill that Lane does. Every screw has a drill on it, and he puts it in with an instrument which works much better than the ordinary screwdriver.

Let me say a word or two in reference to Dr. Smith's paper.

I have seen a number of fractures of the carpal bones. I think I have x-rays of some fifteen cases. The trouble has been that we were taught, when in school, that a fracture had certain definite symptoms, that is, loss of function, deformity, crepitus, and mobility. Not one of these symptoms can be said to apply to these fractures, as brought out in Dr. Smith's paper; and that is the reason we have gone to sleep, so to speak, in reference to this. The old surgeon thought a fracture was never produced except by extreme force. That is not true now, but we were once taught so. We now know it is not true. When we do have a fracture of this kind, the bones, when brought into apposition, will unite. These tarsal and carpal fractures make them different from any other fractures that occur in the body.

Dr. D. S. Fairchild (Clinton, Iowa): The question of the diagnosis and treatment of fractures is one of great importance. Unfortunately, I am in a position where I have to see some of the bad results following the treatment of fractures, or the misfortunes that occur on account of bad results; now, the question arises, Are we treating these fractures as well as we can, and are we getting as good results as we ought to get? The laity evidently think not. How can we do better? By devoting more attention to the diagnosis and the mechanical factors in a particular case. It is probably true that fractures were not created for the purpose of applying plates, but plates are used in keeping the bones in place. For twelve years we have been using these direct-fixation methods, using silver plates, somewhat like those introduced by Sick of Hamburg, and we put them on after the manner suggested by Dr. Black. We got nice results. We do not use them very often, for we have been in this work so long that we have learned some of the mechanical factors involved.

The time was when surgeons knew nothing about or cared anything about or understood anything about the uniting or treating of bones by operation. Now surgery consists of operations, and therefore we must operate on all these cases. If we studied a little more definitely and carefully the mechanical factors and the means by which we can reduce these fractures properly and completely, and then keep them in place, we should get better results. Some of them must be fastened by direct bone-fixation. There is no doubt about that. They cannot all be kept in place without fixation.

There is one other point in connection with this brought out in the paper of Dr. Wetherill, namely, the length of time re-

quired for the regeneration of bone. We have had cases come up for consideration in the courts in relation to deformities that have occurred late. The surgeon has kept on appliances as long as the text-books indicate, then has taken them off and deformity has resulted. I think Dr. Wetherill's paper and the discussions following it are of great importance in calling our attention to this one particular thing. I should say, the difficulties that come up in the treatment of fractures do not come to members of this Association, but they come to the general practitioner, the man who is not eligible for membership in this Association. But we owe a duty to this class of men, and should endeavor to put them in the line of managing these cases in a way that will save the misfortunes that are likely to come upon them. All over the West and Southwest we are hearing of numbers of cases of malpractice suits, and most of them on account of fractures. What are we going to do to prevent them? We must get better results. We have to study all fractures, and I think the American Surgical Association has done something in appointing a committee to consider this question. It would help us more in the way of trouble to ourselves if we study the question of fractures, instead of Jackson's membrane, and so I believe by discussing the question of fractures before this Association and local societies, and eliminating the question of operation on bones until it is shown there is no other way, we shall profit a great deal.

Dr. Samuel C. Plummer (Chicago): In the treatment of fractures by the open method, I think we have had our attention called so much to some of the non-absorbable mechanical appliances, of which the Lane plate is an example, we may forget that sometimes it is sufficient to replace the fragments and not apply anything. I can think of two cases that I treated in this way, one a transverse fracture near the upper part of the shaft of the humerus in a boy, twelve years of age, in which it was impossible to reduce the fragments even under anesthesia. The upper fragment was so short that it would wander around and slide off when you pulled the lower fragment into apposition with it. By simply making an incision and taking hold of the upper fragment to steady it, it was a simple matter to get the other fragment up against it in most accurate apposition. We knew it was absolutely accurate because we could see the little irregularities on the two fragments and how they fitted into each other. When this was done, there was no tendency to displacement; the wound was sewed up, and the case treated as a simple fracture. I expected it would be necessary to wire the bone, but I used absolutely nothing and got a splendid result.

Recently I had a case of fracture of the neck of the radius in a boy, eight years of age, with a dislocation of the head of the radius. The upper fragment was very short. It lay in between the radius and ulna and could not be replaced without cutting down on it. We cut down on it some ten days after the original injury, pushed it back into place, and by sewing up portions of the torn ligament held it. I thought something would be necessary to hold the fragments in apposition, but it did not seem to me there was any inclination to displacement. I did not use anything. I do not know what could have been done unless you took a piece of catgut and held the fragments in place. This seemed unnecessary. Two months later the boy had a good functional result, and, while apposition is not perfect, it will not interfere with function.

In some cases you need something, but not a mechanical appliance. We can get necessary immobilization by the use of absorbable material. For instance, in fracture of the patella I never have used wire or anything like it. Kangaroo tendon or even heavy chromic gut will bring the severed parts together. I do not put this through the bone, but only the torn soft parts, and have had uniformly good results.

I recently had a case of comminuted fracture of the middle of the clavicle, with pressure on the brachial plexus by a fragment which was turned upward. I cut down on this, replaced the fragments, and found another fragment displaced downward. I fastened these together by catgut suture, which surrounded the whole bone and all its fragments, and put them together in that way; and to all appearances the patient will get a good result. In that case we got a slight infection, which cleared up in a few days; but I believe if I had used wire or a Lane plate or any mechanical non-absorbable appliance the slight infection would have caused serious trouble, although in the present case it will not cause any trouble at all.

Dr. John P. Lord (Omaha, Neb.): I want to endorse practically everything that has been said in the papers and in the discussion. I will add one or two points, and they are these: Some ten or fifteen years ago I crystallized my ideas in regard to the perniciousness of introducing foreign material in compound fractures, especially infected ones. The rage for wiring, drilling, and stripping off periosteum and shortening of bone after the removal of fragments, became objectionable, my mind was clarified many years ago in regard to this matter, and it has *not* been my practice to use foreign bodies except to a very limited extent where they cannot be avoided. As a re-

sult of this attitude I have devised what I call an adjustable loop-splint. I devised these loop-splints so that they were adjustable with an adjustable screw at this point (indicating). For instance, a straight one on the back of the leg that would fit against the leg, the upper loop made from these heavy pieces of strap-iron was used and incorporated in the plaster and put in at the dressing. The advantage of these adjustable loops is that the final adjustment of the fragments can be left until the temporary dressing is removed from the wound and the position of the bones reinspected, and then such readjustment made as may be necessary.

I think we should emphasize the advantage of bone-grafts in deficiencies of bone. We should discourage and never practice the shortening of limbs because of deficiencies in bone. Many of these compound fractures have come to us in years past with a number of the fragments removed, and the limb very much shortened, and then an operation for non-union was necessary. In these cases we should make it a point to maintain the length and integrity of the limb, and, if deficiencies of bone exist, to supply these deficiencies by a later operation of bone-grafting after they have become healed and thus preserve a perfect limb.

Dr. Joseph Ransohoff (Cincinnati, Ohio): This subject of fractures has been particularly interesting to me of late because of the fact, that, after doing surgical work during all my professional career, I have at last been sued for malpractice as the result of using a Lane plate on a comminuted fracture of the humerus that could not be maintained in position in any other way. The fixation was not perfect.

I was particularly gratified some days ago, when Mr. Lane was asked to hold a clinic during his visit to Cincinnati, to see him treat a case exactly like my own. After working for an hour and a half in trying to put a plate in, he took it out, sawed off the ends of the bone, put the plate back, and got no better result than we would get from any other method of treatment.

I do not think we ought to plate fractures nearly so often as has been done of late. This is not a new subject. Fourteen years ago I read a paper before the American Surgical Association on plating of fractures that healed in no other way. The plates used at that time antedated those now used. The first plates, used by Schede of Hamburg, were made of ivory, partly absorbable, but in a bone, like the femur or humerus, they were not sufficiently strong to the hold the parts in apposition. I believe in many cases of fracture of the femur it is almost criminal for a surgeon to do an open operation and

plate the fracture. I believe it is one of the most dangerous operations we can undertake, and I am sure that, if the statistics were really fully compiled, a great many more deaths would be reported from open operations in cases of simple fracture of the femur than we have any idea of.

I know one man, a perfectly honest gentleman, Dr. John B. Roberts, who, at a recent meeting of the American Surgical Association, stated that he had had two deaths after plating a fracture of the femur.

I have been asked to state the treatment I have used recently in fractures of the femur and about extension doing good. The old Buck's extension, as it is ordinarily used, has many advantages, as we all know. As Dr. Mayo was sitting next to me during the reading of the paper, I said to him: "Doctor, what would you do if you had a fracture of the femur at the upper third?" He replied, "Stay in bed, with extension."

In the treatment of these cases we have secured extension by means of ice-tongs. It is simple. It is a modification of the Codivilla method of driving a nail through the os calcis, but it has the advantage that it cannot slip. This can be done under cocaine. A small incision can be made on each side of the condyle; with a little hammer the ice-tongs are driven into the bone, and when the weight is put on, it pulls more effectively. The tips of the ice-tongs embed themselves into the bone itself.

Had I known that this discussion was going to take place, I should have brought some *x*-ray plates with me. By using ice-tongs considerable weight can be employed, twenty-five or thirty pounds or even forty pounds in a strong man. Putting the limb on a double inclined plane, the patient is perfectly comfortable, and we can overcome shortening without any difficulty. There is, of course, in addition to the shortening, eversion; and that is more difficult to overcome, but even that can be done.

The more I see of bone-plating in my own practice, the more I see of the *x*-rays exhibited by experts at the various surgical association meetings, and the more I read of bone-plating, the more I have come to the conclusion that there will be less and less of it done, especially by those who are not qualified, either by surgical training or by environment, to do these very difficult operations. But when it comes to the humerus, with the musculospiral nerve about it, less and less will this operation be done there. Let us not forget that a good many excellent results were obtained before bone-plating was ever done. Let us not forget, even if the cosmetic results, as shown by the

x-ray, are not as good as desired, the functional result may still be perfect.

Dr. Haines (closing the discussion): I am not opposed to the use of retaining apparatus in the treatment of fractures. In my paper I pointed out the abuse of the Lane plate and other devices. You will remember, at the Kansas City meeting of this Association last year, there was one man who confessed that he was plating every fracture that came into the hospital. Of course, that is an abuse of the method. Now, he is taking out these plates about as fast as he was putting them in. He has had deaths, and so have other men who are doing this kind of surgical work. I have endeavored to show some of the limitations of this method of treating fractures. The method is not new, as has been pointed out by Dr. Ransohoff, who used it twelve or more years ago.

I do not think we can reiterate too often that this method of treating fractures is being abused by men who are ill prepared in the practical selection and technical work. It is highly technical work and morbidity and death follow in a certain percentage of cases in the hands of the most skillful. My purpose was merely to bring this subject before you and let you thresh it out for yourselves. Our mistakes generally go to the other fellow; but because you have had no bad results in the operative treatment of fractures, do not take the unction to your soul that you have made a great success in treating these cases by this method because your patients are not likely to come back to you; they are more apt to go to some other surgeon.

Finally, are anatomic results always essential in the restoration of broken bones? Every fracture case should be measured by the functional result you obtain rather than by the appearance of the limb. Some very unpromising looking limbs have been restored to full or almost complete function by easy passive motion and massage. Do not over-treat your fracture cases.

THE PRESENT STATUS OF THE RADICAL ABDOMINAL OPERATION FOR CANCER OF THE UTERUS

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On the whole it can be pretty definitely asserted that the radical abdominal operation for cancer of the uterus has never been, and is not now, commonly performed by American surgeons. Since it cannot be claimed that our operators are either less skillful or less intelligent than surgeons in other parts of the world, perforce, there must be another explanation for the failure of the operation to become popular in this country. It will be the purpose of this paper to look at the question from a number of standpoints with a view of determining the reasons for the unpopularity of the operation, and with a plea for a change of conditions, so that the operation under consideration may be more universally employed; for in spite of the discouragement, which comes from not being able to make the primary and end-results of the radical abdominal operation for cancer of the uterus all that could be desired, I am still a firm believer in the operation, which I perform whenever the opportunity arises, in preference to other methods of treatment.

The principal reason for the unpopularity in this country of the radical abdominal operation for uterine cancer is its high primary mortality. Surgeons are not accustomed in these days of highly perfected technic to a primary mortality of from twenty to fifty

per cent, which would be a fair estimate of the mortality of the radical abdominal operation, if all reported and unreported cases could be collected. Surgeons, necessarily, must be content with such a high mortality in emergency operations, neglected cases where the knife holds out the only hope of saving life. Under such circumstances the surgeon consoles himself with the thought that without operation it is sure death for the patient within a few hours or days, and that every life saved is so much gain. However, it is different in the case of the woman with cancer of the uterus, who is able to walk to the operating-room. Such a high primary mortality as has been mentioned in such cases dismays even the most unemotional surgeon, and makes him query if, after all, he is justified in continuing with an operation which carries with it such disastrous primary results. If to this doubt be added a secondary mortality, by no means low, from a recurrence of the cancer, it is not to be wondered at if the surgeon abandons the operation upon what to him seems logical grounds and returns to the safer palliative procedure. I am firmly convinced that, for the reasons set forth above, the personal equation has had much to do with the abandonment of the radical abdominal operation for uterine cancer by American operators; and until two things have been brought about, I believe the operation under discussion will not be generally adopted. First, the profession and laity must be so educated regarding uterine cancer that the disease will be recognized earlier and patients come to the surgeon when local and general conditions combine to bring about a low primary mortality; secondly, for this particular operation true specialization must result, so that the occasional operator will be eliminated.

The profession has been singularly backward in inaugurating the campaign against carcinoma. It has

held aloof for a number of reasons, prominent among which has been the uncertainty as to value of the knife as a means of cure. This doubt on the part of the great body of physicians has its origin in the poor surgical results obtained two or three decades ago from imperfect or incomplete operations where the local disease was not entirely removed. A so-called recurrence after an amputation of a breast for cancer when the axillary glands were not removed, is, in reality, not a recurrence, but a continuation of the disease. The same will apply to cancer wherever situated. But no matter how faulty the reasoning, once let an idea become firmly fixed in the mind of the medical body politic, there it sticks until dislodged by long and continuous efforts on the part of those who have had better opportunities for judging the true condition of affairs.

The pertinacity with which the profession clings to a totally wrong medical conception is illustrated by the erroneous ideas regarding fibroid tumors of the uterus, even now existing to a certain extent. For years it was taught that it was best to tide the woman with a fibroid tumor over the menopause, in the hope that all symptoms would cease after the cessation of menstruation. Time has shown this hope to be without foundation in the large majority of cases. Not only is it true that usually the growth does not shrink, nor do other symptoms, such as increased flow, cease, but the woman is exposed to other dangers even more serious, such as carcinoma of the body of the uterus. Yet so firmly was this idea of the beneficial action of the menopause upon uterine fibroid fixed in the minds of the profession, and through them upon the public, that many a poor woman's life is jeopardized even at this late day through adherence to this faulty treatment.

Hence, first of all, we ourselves must be convinced that cancer is a local disease and capable of cure if

taken in time, and radically removed; otherwise we can never hope to convince our patients and the public in general that there is any cure for cancer.

That this surgical association and others of a similar nature have not long ago united in a campaign against cancer can be explained only upon the basis of there being something which has prevented surgeons, as a body, from beginning the attack. What are the reasons which have held us back when we have seen the great benefits resulting from the antituberculosis campaign? It could not have been that we minimized the importance of cancer in its various forms, for we have known for some time that in certain communities the death-rate from cancer exceeds that from tuberculosis; nor could it have been because we have not had opportunities of seeing and studying the disease, for, from the very nature of the case, the surgeon sees, sooner or later, the vast majority of patients suffering from cancer. Our hesitancy to inaugurate a cancer campaign is probably due to the fact that, unlike tuberculosis, the cause of cancer is still shrouded in mystery, and for that reason we are not in as good a position to enlighten the public as to how the disease is to be avoided. That this is true can be indirectly demonstrated by considering what would happen should the cause of cancer be discovered within the next month. Within six months there would be inaugurated a campaign against the disease fully equaling, if not surpassing, the crusade against tuberculosis. The medical and lay journals would be filled with matters pertaining to the subject, and people would be taught how to avoid this disease, which, for so many years in the public mind, has meant sure death for its possessor.

As a profession we certainly have been remiss in not doing what we can to mitigate the ravages of cancer. While not abating a single effort to ascertain the

cause of the disease, surely it is our duty, individually and collectively, to save by early diagnosis and by surgical means as many of the victims of cancer as we possibly can. Let the people be taught the early symptoms of carcinoma of the uterus, and the results of the operative treatment would soon be improved. If cancer at first be a local disease, as most of us believe, there must be a time when even local, without glandular, removal will result in cure. The impossibility of determining from appearances or macroscopic examination just how long the smallest cancerous lesion has existed or just how far it has extended, will always stand in the way of our remaining satisfied with anything short of radical removal of the disease. But the radical removal, because of the possibility of extension only discoverable through the aid of the microscope, and removal where the gross appearances indicate that the cancerous process has attacked the surrounding tissues, are entirely different. Radical operation under the former conditions can be so perfected as to insure an exceedingly low primary mortality and excellent permanent results; in the other class of cases the primary mortality must always be high, and permanent results poor on account of recurrences.

Thus the present status of the radical operation for cancer of the uterus, at least in this country, is directly dependent upon early diagnosis of the disease under consideration. In my opinion, unless patients can be operated upon earlier, we have about reached the limit of what can be accomplished by the operation, except as the primary and end results may be improved somewhat, as the technical skill of individual operators is increased. This statement has reference to the percentage of absolute cures, which is the reduction to one figure of the ratio between the number of cases of cancer of the uterus seen during a given period, and

the number of patients operated upon by the radical abdominal method, who are alive and free from recurrence after five years. Winter has reduced this to a formula $\frac{O \times D}{100}$, where O denotes the number of patients operated upon out of 100 seeking relief, and D denotes the number of patients remaining healthy out of 100 operated upon.

If, then, in this country we have almost reached the limit of what can be accomplished by the radical abdominal operation for cancer of the uterus, or, in fact, by any operation with the cancer material as it is at the present time, the only way the percentage of absolute cures can be raised is by increasing the value of O, i. e., the number of patients with cancer of the uterus, we are able to operate upon by the method under consideration.

Under the more enlightened conditions of cancer education prevailing abroad, it has been possible to obtain from twenty to thirty per cent of absolute cures of the total number of patients with cancer of the uterus seeking relief. Stated in another way, this would mean that one out of every four women with cancer of the cervix seeking relief can be subjected to the radical abdominal operation, and will be free from the disease at the expiration of five years. That, surely, is something to be striven for when one reflects upon the small number of patients he has been able to save out of the total number he has seen in a given number of years.

In another paper I have gone into the various aspects of the radical abdominal operation for cancer of the uterus. I have reported my results carefully,—my successes and failures. Others have done the same, so that there is now an extensive literature upon the subject. To the unprejudiced mind it has been demonstrated that cancer of the cervix can be cured in a

considerable percentage of cases, provided the patients are seen early enough and the operation is sufficiently extensive. But the fact remains that our results in this country, both primary and permanent, are exceedingly poor, and that it is time we began the campaign of education on carcinoma in general, and, because of its great frequency, cancer of the uterus in particular. While, primarily, this paper is a plea for the necessity of early recognition of cancer of the uterus, in order that the status of the radical operation may be improved, it seems to me it is the duty of this society to broaden the campaign so that it will include all kinds of carcinoma which can be benefited by surgery. In other words, I would have this Association, through a committee appointed at this meeting, devise some plan by which it would be possible to secure coöperative action upon this question by all the special surgical and gynecological societies in America, including the surgical and gynecological sections of the American Medical Association. After the whole subject has been thoroughly gone into, and the plan of action determined upon, it will be a very easy matter to interest the various state associations in the question. Just how the matter should be worked out is not particularly important here. The main things to be emphasized are the poor results now obtained in the operative treatment of carcinoma; the possibility of improving the results of such treatment by earlier recognition of the disease, by both the profession and the laity; the necessity of inaugurating a campaign of education on carcinoma through committees appointed from the various national surgical and gynecological societies; and, finally, the desirability of having this Association be the pioneer in this movement.

DISCUSSION

Dr. Walter B. Dorsett (St. Louis, Mo.): We all have our own individual ideas and opinions as to the advisability of

adopting any particular surgical procedure in any given case or any particular disease. To my mind the radical operation for cancer of the uterus (the Wertheim operation) is ideal, and I dare say it would be much more successful if performed by men who are capable of doing good surgical work; but we must take into consideration the fact that all the members of this Association and all the members of the special surgical societies combined would be a small army opposed to the ravages of cancer, so that the personal equation enters very largely into the consideration of this subject.

Unfortunately, as Dr. Peterson has indicated, we get these cases in the very worst form. It is rather seldom that a woman comes to a gynecologist or a general surgeon with a cancer in its incipiency, and one of the saddest things is to find these cases that are inoperable and we have to make the patients acquainted with that fact and with our opinion. Now and then patients come to us whose cases appear to be inoperable, that is to say, on examination we find a large crater, with total absence probably of the vaginal cervix, and with some involvement perhaps of the posterior wall of the bladder and some indication of attachment to the rectum; but, still, now and then, if those cases are operated upon they get well. I have in mind now one case in particular in which I started to do a vaginal hysterectomy and took hold of the anterior lip of the uterus with a pair of forceps, and it pulled off, and finally I got a barbed-shaped instrument, introduced it into the uteurs, and the whole uterus fell out. I simply cauterized the rim with the actual cautery. That was many years ago,—I think twelve years ago, —and I told the attending physician that he had better take the woman home, give her as much morphine as she could take, and let her die easily; but she is living today. In this case I simply happened to get on the outside of all infiltrated tissues, and she had no metastasis beyond the site at which the effect of the burn penetrated. Two such cases have occurred in my own work, so that we cannot say always that this or that case is or is not operable. At the same time, we should lay down certain rules and follow them as carefully as possible in the elimination of the cases that are to be operated on from those that should not be operated on.

I have here some fragmentary notes which may be of interest and may contribute somewhat to the discussion of this subject.

Pryor found that the mortality-rate of the radical operation in 98 cases done by Ries, Werder, Polk, Russel, Clark, and himself was 11 per cent. To these are added 2 successful cases of Pryor, 2 successful cases of Ries, 25 cases with 3 deaths by

Wertheim, and 15 by himself with 1 death, totaling 142 operations with a death-rate of 10 per cent.

Operations by the men mentioned above other than Wertheim were almost as radical as the extensive Wertheim operation. On the other hand, many cases have been reported in which at autopsy death was found to be due primarily to the operation; that some of the glands were overlooked, and these glands were located along the ureter or along the bifurcation of the aorta. Many of the glands when examined were not found to be cancerous. They were found to be more or less inflammatory, and many of these glands when removed and submitted to the microscopist were designated as non-cancerous glands, simply inflammatory, so that in doing an operation we are not able to say with the gland appearing under the finger whether it is cancerous or not.

Russel investigated the after-condition of 48 cases of vaginal hysterectomy for cancer of the cervix, and found that almost invariably there was recurrence at the site of the scar, and not in the region of the lymphatic glands. That, it seems to me, is an argument against the radical operation for cancer of the uterus.

Wertheim, in 60 operated cases, found involvement of the removed glands in 15 per cent of early cases and in 31.7 per cent of all cases. Schauta's autopsy study of 60 cases, in 40 of which the patients died from the natural effects of the cancer, and in 9 from intercurrent affections. In 43.3 per cent of the whole series the glands were entirely free of carcinoma metastases. The lower removable glands alone were involved in 13.3 per cent, the upper (not removable) glands alone in 8.3 per cent, and both lower and upper glands in 35 per cent.

Kundradt, in 76 cases operated on by Wertheim, in which the parametrium was involved on one or both sides, found the glands entirely free of metastases in 71 per cent. The glands on one side were involved in 22 per cent, and the glands on both sides were involved in 7 per cent. In his analysis of 80 cases he found only 4 in which the glands were involved with the parametrium free. Winter found cancerous glands in only 2 cases in 44 autopsies on patients where the disease was confined to the uterus. Sometimes we see these patients where the disease is apparently confined to the uterus, so that the conclusion as to the operability and non-operability may be summed up about as follows:

A. Cases which are operable:

1. Where malignant disease is limited to tissues which can be removed completely.

2. Where the disease is confined to the uterus.

3. Where the carcinoma has slightly invaded the parametrium, if the connective tissue is removed as far out as possible from the uterus.

4. Where the uterus is carcinomatous and is immovable, but the fixation is due to some inflammatory condition other than being fixed by malignant infiltration.

5. Where the uterus is pregnant, and the disease has not invaded too far, but where there is a fair chance regardless of the fetus.

6. Where there may be involvement as far as to take in portions of the bladder and the lower part of the ureter, but the tumor is of slow growth and seems localized, and where some would advise the removal of a portion of the bladder and ureter, but this is exceptional. So far as my own personal choice is concerned, I would consider where the bladder is not involved, which can be ascertained by cystoscopic examination, as well as by vaginal touch, and in those cases in which we have the rectum clear and by rectal touch or by examination with the proctoscope can determine the condition to be not affected, these cases are operable.

B. Cases which are inoperable:

1. Where the uterus is immovable, due to malignant infiltration.

2. Where the malignant infiltration involves the greater portion of the bladder, ureters, and rectum, including other structures, making complete removal impossible.

3. Where the cancerous condition may be operable, but the general condition of the patient forbids operation, due to serious disease of the heart, kidneys, or lungs.

4. Where the uterus is pregnant, but the disease is too far advanced to render the operation safe to the mother, in which case palliative measures are advisable in attempting to bring the child near term for some mode of delivery.

Exploratory laparotomy is advisable in cases to determine operability by radical measures, for the reason that in two cases of my own, while the disease seemed to be limited to only a small spot on the posterior neck of the uterus, with a slight involvement of the posterior vaginal fornix, I have found that I had disseminated cancer in the broad ligament, metastasis even in the broad ligament, and we cannot always tell from the vaginal examination with the speculum or digital touch how far the disease has advanced. Another argument

which might be used in reference to making an abdominal incision is, that the operation can be done by the combined method with more despatch and with less danger of soiling the peritoneum or implanting disease germs on the peritoneum. My method generally is to dissect out the bladder anteriorly, make a good wide incision into the cul-de-sac of Douglas, secure the uterine arteries on either side with clamps, open the abdomen, and remove the balance by cutting loose the uterus from above and drawing it down, even if we have to make quite a slit in the vagina, dilating it as much as possible, and then draw it down through the vaginal tract, and in that way save soiling of the peritoneum.

Note.—I wish to acknowledge my indebtedness to Dr. J. C. Landree for gathering the statistics here quoted.

I am in hearty accord with Dr. Peterson in regard to the campaign of education against cancer, and I hope this Association will take steps along the line indicated in the paper or in some other manner, whether it be a committee from this Association conferring with like committees of other societies, or whether it is an endorsement of what has taken place in other societies. I think we ought to be more thoroughly in touch with other societies, and it is possible we can serve them better through a committee from this Association than to endorse something that somebody else has done.

Dr. James F. Percy (Galesburg, Ill.): I am very much interested in this subject, and especially from the standpoint of Dr. Peterson's paper. As you know, Dr. Peterson's paper covers the 25 per cent of operable cases, and that 25 per cent in the hands of men who can operate, as Dr. Peterson can, who are given probably the best opportunity that they can have of getting results from operations. What are you going to do with the 75 per cent? Dr. Peterson makes a plea to educate both the public and the profession, and I am sure the profession needs educating as much as the public.

A woman, thirty-two years of age, came to me a few weeks ago with a cauliflower growth of the cervix that was at least six or seven inches in diameter. There was a great crater, such as that referred to by Dr. Dorsett in his discussion, and the only reason why she stopped taking local treatments from three different physicians was because the hemorrhage following the local treatment was so profuse she despaired of living. She was afraid she might bleed to death. Now, we are up against the proposition of dealing with the 75 per cent, and I simply want to call attention again to the paper I read before this As-

sociation last year relative to the value of heat in these cases. My list of cases of this type is growing rather rapidly now. I have four in my hospital that have been under this treatment, and the more I see of it, the more I am convinced that it is the only hope so far these patients have, and I can shorten my discussion and epitomize the subject by describing a case that I operated on two months ago, if you call this an operation.

A woman had the Wertheim operation performed on her by one of the best men in this country last April. I saw her in August with the pelvis packed full of cancer, the lower abdominal wall extended perhaps four inches above the pubes and was elevated two inches above the surface of the pubes by cancer. She weighed about 90 pounds. She was bedridden and cachectic, and looked as though she was going to die. She was seen by a member of this Association, one of the best surgeons in this country, who simply threw up his hands and said that absolutely nothing could be done for her. She came under my care and I was asked if I could do anything for her, and I said I did not know. I said to myself, if there is anything in the heat proposition as being inimical to the growth of the carcinoma cell, here certainly is a case to try it on. I used the large electric cautery, as you remember last year, with its tip an inch and a half long, and two and a half inches in diameter. I opened the abdomen, and pushed this cautery through the vault of the vagina, or what was left of it, and kept that cautery in the pelvis for forty minutes. The cautery, when I removed it, registered 155° F. I went through the center of that mass, and my assistant's fingers kept me from going through the mass. I reduced the thickness of the walls of that mass to about one-quarter inch, so that I could squeeze it together, and I did the same thing in the uterus and cervix. What I am after is heat. I almost made a hole in the sigmoid in this woman, and at the present time the bowel movements are coming through the vagina. But I do not care anything about that, because it is the lesser of two evils, and the same thing is true in reference to the carcinoma in the bladder. You can look with the cystoscope, as pointed out by Dr. Dorsett, into the bladder, as I did in one of my cases, and the carcinomatous mass was almost two inches along the floor of the bladder. I burned that all out. I burned a hole through the bladder. If you burn into the bladder through the uterus, the chances are that contraction takes place and the uterovesical fistula will close. I have had three cases that have done that.

In this particular case I speak of, at the present time the woman is apparently getting well. Her appetite has come back,

and she is eating enormous quantities of food. I may say, this growth extended to the rectum, so that the mucous membrane in the rectum was not movable, which, I believe, is one of the best indications that you have cancer. The inflammatory mass did not hold the mucous membrane down. There was an ulcer as large as a ten-cent piece in the rectum. That has all disappeared. The only thing that is left today is a mass high up in the rectum about half the size of a hen's egg, and that is not painful. It seems to be cicatricial. At any rate, I am going to open the abdomen again and cauterize that spot.

As I have said, this woman apparently is getting well. She has gotten over her edema of the tissues, and has gotten over everything in the way of bad symptoms. This adds interest to the report of Lambert in the last issue of the *Jour. of the A. M. A.,* who speaks in his article of the hypersensitiveness of the carcinoma cell to heat. He reiterates the language that was used in my paper, which I learned from Loeb of the Woman's Skin and Cancer Hospital in St. Louis, namely, the carcinoma cell in the mouse will not stand a temperature six degrees above normal for any length of time, and you cannot transplant it, while the frozen section 40° below zero, kept for many hours, can be thawed out and transplanted. In other words, the carcinoma cell or organism, whatever it is, is evidently affected easily by the heat, and the thing that must be done is simply to raise the temperature in the pelvis with the least destruction of the tissues that you do not want to destroy, if possible, but where you destroy some of them, it is the lesser of two evils.

Dr. W. D. Haines (Cincinnati, Ohio): In the first place, I feel that probably our basic proposition is at fault, and, until we can find out the cause of cancer, we are going to grope around in the dark in our efforts to effect a cure. The cure of cancer is on an empirical basis today, according to our clinical experience, and that is all we can say of it.

I cannot but feel that there are great differences in the degree of malignancy in various cases. For instance, take the cases referred to by Dr. Dorsett in opening the discussion and those considered by the essayist. I believe these cases will show that there is a wide difference in the degree of malignancy.

I reported to this Association some years ago a case that came under my care in 1899, in which we thought it was absolutely hopeless to try to do anything. In this instance, however, in order to check the terrible hemorrhages which were sapping away the woman's life, I took the ordinary cautery, somewhat after the fashion that the tinner uses a soldering-iron and

cauterized the uterus thoroughly. That woman was alive and well last year. I have not seen her this year. I went well into the cavity of the uterus, not with a view of curing her, but merely of checking the hemorrhage.

With reference to the glandular invasion: Take a man who would remove a carcinoma of the lip, of the breast, of the glans penis, or of any other part of the body, without dissecting the gland-bearing tissues in that vicinity, and we would say he had done a very incomplete operation. So we come to the point that the man who does not remove the lymphatics does not have a recurrence of malignancy, but simply has a continuation of it, and I believe, gentlemen, in dealing with carcinoma of the uterus we must include this in our protocols on the treatment of cancer of this organ. We must resect the gland-bearing tissue if we are going to do a complete operation for malignancy, as we understand the operation today.

Dr. William Jepson (Sioux City, Iowa): I want to ask your attention for a moment because of the statement made by the last speaker; namely, owing to the fact that we do not know what the cause of cancer is, we are beating about the bush, so to say, and are not able to do very much if anything.

I do not wish to enter into the consideration of the treatment of cancer from a scientific standpoint other than to add support to what Dr. Peterson has said relative to the necessity of having the profession, as well as the people, understand the importance of lesions leading to cancer. "I wish to add support to the statements made by Dr. Peterson, that every malignant growth is amenable to cure, provided its recognition can be made sufficiently early, and is so located that it is accessible." Gentlemen, it is said that the mammæ represent 9 per cent of the total cases of cancer; the uterus represents 15 per cent, and the skin approximately 4 per cent. There are approximately 40,000 people dying every year in the United States of cancer. Think of it. This is a mortality that is only second to that of what? Tuberculosis. Tuberculosis is subsiding annually at a rate approximately from fifteen hundred to two thousand; while cancer, on the other hand, is increasing. I do not mean to say it is increasing in a sense because it is infectious, but we are becoming more and more capable of recognizing its presence. If it is true that cancer of the breast, cancer of the uterus, cancer of the lip, or cancer in any other part of the body, is capable of being removed when diagnosed in the early stage, is it not criminal for the family physician, during the critical period of a woman's existence, when she is terminating child-

bearing and has reached the age of forty or forty-five, not to inform her that the breasts and the uterus now exist only as vestigial structures and are prone to cancer and should be examined for conditions tending to its development? In fact, I question whether any family practitioner is doing justice to a family, and to the poor mother, not to inform her of the danger in passing through that period. The general practitioner must be alive to a great many things, among which are the predisposing causes of cancer of the uterus, for an unrepaired laceration of the cervix, or the reaction to a constant, irritant, which is represented by an inflammatory process. These conditions should be cared for. He may find in the breast similarly the presence of a growth which up to that time has been of no significance to the woman. Therefore I cannot help but feel that the time has come for us to impress upon the general practitioner the necessity of acquainting his patients with the dangers of harboring growths, whether benign or not, or harboring unrepaired solutions of continuity when that individual is entering the period of senescence. It does not matter what the cause of cancer may be, whether it is a protozoön or bacterium, for, until it is possible for us to destroy this by some antitoxin, these women must be subjected to operation for the removal of the cancer, for it would be a shame to allow them to go on to the grave between now and the time when we shall doubtless have a remedy for this disease. With four thousand people dying annually of cancer of the uterus, and two thousand from cancer of the breast, we are, to a certain extent, responsible for it. We are not doing our duty to the people. Why should we as surgeons in studying the scientific basis of operative procedures neglect the opportunity to acquaint the public with the fact that this malady is taking away annually such a large number of human lives that may be saved by timely interference. We should appoint a committee or do something in order to discharge our obligations to humanity.

Dr. Peterson (closing the discussion): It was not the purpose of the paper to discuss the radical treatment of uterine cancer. I simply wished to emphasize the fact that added dexterity, gained from experience with this operation, can do but little to improve primary or end results unless the patients with this disease can be seen earlier.

I consider Dr. Percy's method of treating with the cautery the 75 per cent of cases of uterine carcinoma which can not be subjected to the radical operation exceedingly valuable. He has been kind enough to demonstrate this method in my clinic

at the University of Michigan, and I am preparing to give it a thorough trial in the large number of radically inoperable cases entering the clinic each year. I have, however, proceeded far enough with the method to satisfy myself that the cautery treatment by his method is much better than the chloride-of-zinc-packing method I have used up to this time. Of course, except for a case here and there of the basal cell type, the carcinoma will return, but many lives will be prolonged and made endurable by means of this particular cautery treatment.

It is for the Association to say how the campaign of education on carcinoma shall be inaugurated. While the movement of educating the laity by means of magazine articles on cancer may be a step in the right direction, and possibly will accomplish much, I also believe that the special surgical and gynecological societies can do much toward educating the profession along these lines.

PREGNANCY IN THE RUDIMENTARY CORNU OF UTERUS UNICORNIS, AND THE REPORT OF A CASE WITH FULL-TERM FETUS.

Eric P. Quain, M. D.

BISMARCK, NORTH DAKOTA

Defective union between the two Müllerian ducts in embryo results in various uterine anomalies. A total and permanent separation of the ducts, with the development of a separate uterus tube and ovary from each, is termed a uterus didelphus, or uterus duplex separatus.

The ducts may fuse in the lower segment of the uterus and not in the upper, and a uterus bicornis is the result. Should the fusion be complete for the outer wall of the uterus, but leave a septum between two cavities, we have a uterus septus, or uterus bilocularis.

Again, one Müllerian duct may fail to take any part in the formation of the uterus, while the other duct has formed a complete uterus with a normal tube and ovary. Such an anomaly is termed uterus unicornis. The duct on the defective side, however, with rare exceptions, does form a fairly normal ovary and tube at its upper extremity. The connection between these and the uterus is indicated by a bundle of muscle fibers, often arranged in layers like normal myometrium. Within this muscle tissue lies a small cavity, lined with mucous membrane of endometrial

type. The mucous membrane is continuous with that of the tube, but does not communicate with the uterus in more than 20 per cent of the cases observed. The round ligament joins the muscle fibers near the point where the tube is attached. Into this rudimentary cornu of a uterus unicornis may be deposited a fertilized ovum. In those cases where an open duct does not connect tube and uterus, this impregnation can take place only as a result of transperitoneal migration of the fertilized ovum or of the spermatozoön. The following case from our experience will serve to illustrate these conditions:

Mrs. H., aged 21, German, and of excellent family history, came to me for consultation February 28, 1911.

Personal History.—Never ill; menses at 17; regular every 26th day and painless; married in December, 1909; last menstruation March 29, 1910, and only slight traces at this time. She had some headache and dizziness during the following two months, but no nausea nor unusual abdominal symptoms. The abdomen enlarged gradually, and life was felt in the beginning of August. Fetal movements were noted daily from this time until December 29. On this date she had severe intermittent pains in the lower abdomen, and expelled from the vagina a "piece of flesh the size of two fingers," and a small amount of blood. After this no fetal movements were felt. On January 4, 1911, she again suffered from intermittent attacks of pain, and passed a small amount of blood-clots. Since then she had suffered considerably from abdominal tenderness, but had been able to do most of her housework. Her abdomen, previously much enlarged, had gradually decreased in size, and a hard tumor had developed near the umbilicus. Her breasts, large and distended with milk about January 1, had decreased in size.

Examination showed a medium-sized young woman of apparently perfect development; pulse and temperature were normal; heart and lungs, normal; breasts, large but flabby, and contained milk; abdomen, enlarged from a tumor extending from the pelvis to a point slightly above the umbilicus. The tumor was even in outline and had the position, shape, and size of a pregnant uterus in the seventh month, but was quite tender and presented a striking woody hardness to the palpating hand. The vaginal mucous membrane was pale, and the cervix hard, closed, and of normal size. A normal-sized uterus was located below and to the left of the tumor, but attached to it by a thick firm band. The lower pole of the tumor was less tender than the upper parts. Uterine bruit and fetal-heart sounds were absent. The urine showed a slight trace of albumin; leucocytosis was 10,000. A diagnosis was made of ectopic pregnancy with dead full-term fetus.

Operation.—On opening the abdomen and exposing the tumor, the omentum was found adherent to the anterior wall of the tumor over two small necrotic areas, each the size of a five-cent piece. A dark brown grumous fluid escaped through these areas after separating the omentum. No other adhesions were present. The right tube was stretched across the upper part of the tumor, and the right ovary was attached posteriorly in normal relation to the tube. The tumor was attached to the right side of the pelvic floor by means of a broad, fleshy, and very vascular pedicle. The round ligament, entering the tumor laterally about its middle, corresponded to the external margin of the pedicle, while the shorter and thicker mesial margin was attached to a normal-sized uterus about 3 cm. below the fundus. The round ligament was attached near the inner ostium of the tube. The left adnexa appeared normal, the ovary being much smaller than the right. From the point of attachment

of the pedicle to the right margin of the uterus, a membrane extended to the rectum, dividing and partly obliterating Douglas's cul-de-sac.

The tumor, aside from its hardness, had the general appearance of a pregnant uterus, with the right tube, ovary, and round ligament situated in a relatively high position, but lacking the left adnexa. The uterus, packed into the left compartment of the cul-de-sac, appeared as a normal non-pregnant uterus with normal left tube, ovary, and broad and round ligaments, but minus the right adnexa, save the 3 cm. long and half as thick pedicle passing from the lower uterine segment upward into the tumor. The pedicle was clamped, and the tumor, together with the tube and ovary, was removed. The blood-vessels were ligated, and the peritoneal wound sutured. The abdominal wound was closed, and the patient recovered without complications.

The specimen removed was hard and globular and 22 cm. in diameter. The tube was 15 cm. long, the inner third of which passed obliquely through the tumor wall, while the outer two-thirds were free. It was attached across the external surface and near the top of the tumor. The ovary was large and distended from a cyst, which was found to be a corpus luteum. The round ligament was attached 5 cm. below the inner ostium of the tube. A triangular raw surface, about 10 cm. to each side, showed the place of excision through the pedicle. The main blood-vessels entered near the outer margin of the pedicle. (Fig. 1.)

On incising the tumor, a dead, partly macerated, and tightly compressed fetus escaped with some force from the retracting muscular edges of the incision. It occupied a sacroposterior position with breech presentation. A small amount of thick brownish fluid was contained in the amniotic sac. The thin and necrotic placenta, weighing 300 gm., was partly loos-

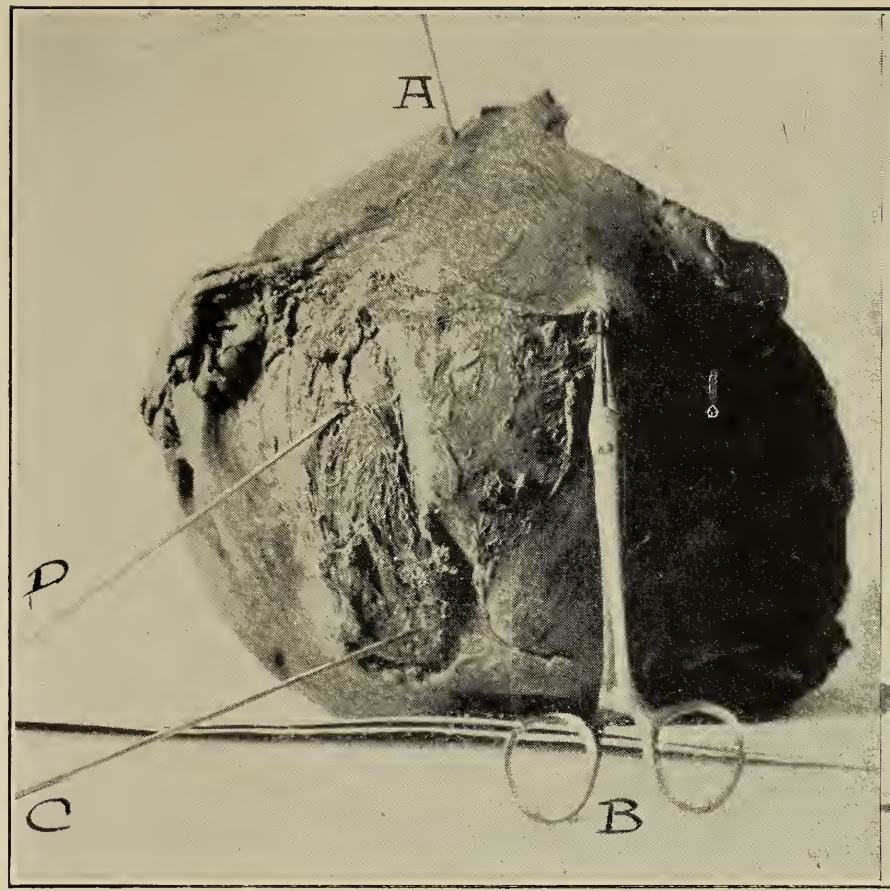

Fig. 1. A, probe; B, forceps on round ligament; C, probe at point of attachment to uterus unicornis; D, probe at point of entrance of main blood-vessels.

Fig. 2. Fetus in cyst with margin of cyst everted. Shows necrotic areas and exposed bones on fetus; also edges of placenta.

Fig. 3. Skiagram of fetus, showing bone development.

ened from the cyst-wall and studded with old blood-clots. It had a butterfly outline, with the cord 34 cm. in length, attached between the two wings. It was spread over fully two-thirds of the cyst-wall. It did not reach the top of the cavity, where the necrotic perforations had taken place. These perforations corresponded to the left knee and elbow of the fetus.

The cyst-wall had a thickness of 1.5 cm. over its inferior and external surfaces. The superior surface was scarcely .5 cm. in thickness. Its weight, after removal of fetus and placenta, was 424 gm. Numerous heavier muscle-bundles ran in various directions within the wall, leaving thinner spaces between them. No opening or communication existed between this fetal cyst and the uterus. Microscopic sections made from the pedicle failed to show anything but bundles of muscle fibers with some round-cell infiltration and a few blood-vessels. The lining of the cyst was too necrotic to show the structure of the mucous membrane. Sections of the wall showed the muscle fibers in the thicker portions to be arranged in strata.

The fetus, a female, was 45 cm. long and weighed 1,915 gm. It was covered with vernix caseosa. Hair, 2 to 3 cm. in length, covered the scalp. The finger-nails extended beyond the finger-tips. One knee, one shoulder, and both elbows were necrotic, the bones protruding through the skin at these points. These parts had been in contact with the contracting muscular cyst-wall. (Fig. 2.)

After these observations we were compelled to modify our diagnosis of ectopic pregnancy and to specify pregnancy in the rudimentary cornu of a uterus unicornis. Additional interest was gained in this instance by the fact that the fetus had unquestionably lived to term. This is shown not only by the history of the pregnancy, but also by the physical

findings on the examination of the fetus, as stated above. Skiagraphs, showing the advanced bone-development and especially the centers of ossification in the lower epiphyses of the femur, add further evidence to this argument. (Fig. 3.) (See Edgar's Practice of Obstetrics; Olshausen & Veit's Geburtshülfe; Heisler's Text-book of Embryology.) That a transperitoneal migration of the spermatozoön, or transmigratio seminis externa, had taken place is established by the finding of the corpus luteum on the same side as the impregnated impervious rudimentary cornu.

A search through the literature disclosed the fact that up to date about 150 cases of this type of ectopic pregnancy have been reported, either directly or referred to indirectly by various authors. Surprisingly few are the reports from American surgeons. It was found practically impossible to classify in a scientific and useful manner the cases reported, and for the reason, chiefly, that the reports in many instances were quite incomplete. The confusion of terms used in describing congenital uterine anomalies is another obstacle in this connection.

Kehrer,[1] in 1900, described 82 cases of pregnancy in the rudimentary horn. Cohn,[2] in 1909, added 50 more, but complained that many cases were questionable on account of the confusing nomenclature. Guillaume,[3] in 1911, tabulated 145, but does not claim to have exhausted all the literature on the subject; moreover, a study of his work tends to show that he has undoubtedly included several cases which really were uterus bicornis or uterus septus. It would seem advisable to confine the term *uterus unicornis* to those cases which have a demonstrable pedicle between the uterus and the cavity in the rudimentary cornu, where the fetal sac develops. This pedicle may or may not contain a duct.

Of the reported cases about 30 are said to have

gone to full term (see references 4 to 35); but a closer study of these advanced cases has convinced the writer that this number is much too high. Several authors have described a dead fetus which had "died at term" while, as a matter of fact, either the history would tend to show that the fetus died a month or more before full term or the description of the specimen would fail to prove a development corresponding to full term. Again, the brevity of detail in many of the reported cases prevents us from proving the actual number of full-term pregnancies in the rudimentary cornu on record. This diagnosis must rest very largely on the examination of the fetus, for it is often impossible to state accurately from the history given by the mother even the approximate time when the fetus died. This is true especially when symptoms of distress are present both before and after the death of the fetus. The actual size of the fetus is of less importance than the development of special tissues and organs. In nearly all the reported cases of unquestionable full term fetus, the measurements have been below the average for normal pregnancy, while the other signs of full term have been present. This may be accounted for by the excessive pressure within the abnormal fetal sac, and by the deficient blood supply.

When a cavity exists in the rudimentary cornu it has always been found communicating with the tube and to contain a mucosa surrounded by muscle fibers of a uterine type. The impregnated ovum finds therefore a very congenial soil for development. Fertilization has taken place by transperitoneal migration of the spermatozoön in at least half the cases observed. The pregnancy continues, as a rule, until the sac ruptures. The mucous membrane may have defective areas, allowing the chorionic villi to penetrate into the muscle tissue, and to cause intraparietal hemorrhages and perforation in a manner similar to that of tubal rupture.

The evolution of this variety of ectopic pregnancy is usually quite benign, and it is exceptional that the patient has any symptoms beyond those of normal pregnancy. The rudimentary structure has given no sign of its presence beforehand, and the woman may have had normal pregnancies in the uterus unicornis. The only abnormality noted before the termination of the pregnancy in several cases has been the lateral and somewhat oblique position of the fetal tumor.

If the pregnancy goes to term, labor-pains set in. They are less frequent and less severe than in normal labor and result in nothing more visible than some blood-stained discharge or perhaps the expulsion of the uterine decidua. The fetus dies after the first few pains. After the death of the fetus the fetal tumor decreases rapidly in size from the absorption of the fluids. After the cyst-wall is firmly contracted about the fetus and placenta, leucocytic infiltration helps to give the tumor a hardness which can be best described by the adjective "woody." A sudden rupture of the sac may follow the contractions and result in intraperitoneal hemorrhage and the escape of the fetus into the abdominal cavity.

The most frequently encountered complication in this condition is a rupture of the gravid sac. It happens most often about the fourth or fifth month. It may happen any time after this period, but very rarely before the third month. A sudden rupture does not usually cause as severe intraperitoneal hemorrhage as the corresponding rupture of a tube and for the reason that the muscular cyst-walls retract, compress the torn blood-vessels, and grasp firmly the contained placenta, after the escape of the fetus. A fetus thus expelled from the cyst may live in the abdomen for an indefinite period. It is interesting that the only reported living child delivered from this variety of ectopic pregnancy was removed from the abdominal

cavity 19 days after the rupture of the fetal sac. (V. Ott[36].) A second case cited by Cohn from Reidinger, in my opinion, was a double uterus and not a uterus unicornis. Occasionally, a more chronic perforation takes place at a point of long-continued pressure against some prominent part of the fetus. The amniotic fluid then escapes into the abdomen and allows the cyst-wall to contract firmly upon the retained fetus. Another less serious complication is the premature death of the ovum within the rudimentary organ. This may be caused by intraparietal hemorrhage due to penetration of the chorionic villi or to excessive intracystic pressure.

Retention of the dead fetus will result in either of the above-described complications. If the mother survives the rupture of the sac and the death of the ovum, an absorption of the fluids takes place, and the fetus, placenta, and membranes become walled off by peritoneal adhesions. The toleration for this foreign body within the abdomen has scarcely a parallel in pathology. Infection and suppuration have proved fatal to the mother in some instances, but more frequently, maceration and absorption of the soft parts occur. More unusual results are mummification of the fetus and transformation into a lithopedion. In several instances necrosis and perforation of adherent organs have caused bones of a retained fetus to escape into the bowel and into the bladder, many years after the time of gestation.

A correct diagnosis of pregnancy in the rudimentary cornu has been made in but a few instances before operation. (Draghiesco,[37] Hoene,[38] Lindner,[18] Knauer,[39] Wehle.[40]) The usual diagnosis made is extra-uterine pregnancy. In view of the similarity of treatment this mistake is not serious. To call this abnormality a uterine fibroid or an ovarian cyst, as has been done a few times, might have dire results. The important diagnostic points to determine are:

1. Whether or not a pregnancy exists.

2. Whether the uterus unicornis can be palpated as distinct from the tumor, but attached to it by a pedicle opposite the internal os.

3. Whether intermittent contractions in the rudimentary cyst-wall be palpable, as in a normal pregnant uterus.

4. A possible palpation of the round ligament external to the tumor, which would eliminate tubal pregnancy.

The prognosis is better than in other varieties of ectopic pregnancy, owing to the fact that the whole fetal sac usually can be rapidly and safely removed by clamping and ligating the pedicle. The operations performed within the time of aseptic surgery have been eminently satisfactory.

The treatment is that of ectopic pregnancy in general. The removal of the gravid rudimentary cornu would be indicated when diagnosed, whether the fetus were dead or alive. If diagnosed in the seventh or eighth month, the question might be raised whether it would be advisable to wait in an attempt to save the child. It is to be remembered that a pregnancy in a rudimentary cornu is less liable to rupture than are the other varieties of ectopic pregnancy. But no temporizing should be practiced unless the patient be put to bed and be within instant reach of the operating-room. At operation it would seem wrong to exercise a sterilizing technic on the normal tube. Normal pregnancies have followed in uterus unicornis after removal of the rudimentary side, and no instance of tubal pregnancy is recorded.

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VENTRAL TUMORS OF THE SACRUM

ARTHUR AYER LAW, M. D.

MINNEAPOLIS, MINNESOTA

Embryology gives us the key to a proper understanding of sacrococcygeal tumors,—those growths ventral to the sacrum, or the post-rectal tumors, so called. This region is prone to tumors of a different type from those usually found elsewhere, viz., dermoid cysts, teratomata, and mixed tumors. The common tumors of otherwise wide distribution are rare here, and we shall therefore limit our study to the above-mentioned tumors peculiar to this locality.

Consideration of the caudal end of the spinal axis in greater detail clears up some of the haziness which has, in the past, surrounded our understanding of these growths. When, early in embryonic life, the entoderm forms the caudal intestine, the canal dorsalis, and the chorda dorsalis; the mesoderm, the connective tissue, muscles, vertebræ, and blood-vessels; and the ectoderm, the primitive streak, medullary tube, and its vestiges, the central canal of the spinal cord and the primitive alimentary canal are continuous round the caudal extremity of the notochord. This passage, which brings the developing gut and cord into communication, is called the neurenteric canal.

When the proctodeum invaginates to form part of the cloacal chamber, it meets the gut at a point in front of and some distance above the place where the neurenteric canal has its opening into it; therefore there remains, for a period, a part of the gut down be-

hind the anus, called the post-anal gut. This is later obliterated, as is the neurenteric canal.

Considering the complexity of the embryonic tissues forming the caudal section of the trunk axis and their perfect evolution ere they arrive at maturity, we can understand how many and varied deviations from the normal are possible; indeed, the marvel is how we so frequently escape the pathological forms.

We know that the simpler dermoids are most frequently situated in the lines where, during embryonic life, coalescence takes place between cutaneous surfaces. These are designated "sequestration dermoids." A rarer type arises in what are called obsolete canals, and these are called "tubulodermoids." Tumors belonging to this genus are found only on the head, neck, and trunk.

The more complex tumors, the teratomata or the mixed tumors, are reported in the sacral region, in the brain, bladder, ovary, testicle, thorax, and peritoneum. Ekehorn collected 31 cases of mediastinal dermoids, many of which showed the three fetal layers once believed to be necessary in the true teratomatous growths. These reports refute Wilm's original assertion that true teratomata could be found only in the ovary and testicle. He erroneously placed them in a class by themselves.

The Marchand-Bonnet theory, which accords a genetic equality to all composite dermoids, embryos, and parasites, is still generally accepted. Borst, in commenting on the ventral tumors of the sacrococcygeal region, says: "As we sometimes have such great disturbances in this region, possibly in stunted conditions of the lower end of the axis skeleton, portions of the vestiges coccygientes may be transplanted to the ventral side and give rise to tumors."

Middledorpf was the first observer to clearly associate these congenital tumors with the post-anal gut.

Fig. 1. Section of large tumor, showing alternating areas of hemorrhage and necrosis, with one focus of calcification.

Fig. 2. Area in large tumor showing diffuse infiltration of embryonic cells.

Fetal rests from the obliterated bowel form the nidus for the growths.

The literature is rich in various theories as to the cause of these tumors. Much is, as yet, speculation as to their etiology, still pathologists and embryologists are generally agreed that teratomata may arise from local disturbances of development of misplaced tissue anlage, known as monogerminal tissue-implantations.

The simpler dermoids, with their sebaceous material, hair, skin, sweat glands, teeth, etc., are apparently due to the simpler inclusions of the ectoderm. The teratomata, however, generally show evidences of all three fetal layers, as may the mixed tumors. Middledorpf, Ritsch, and Masse believe the tumors under discussion are due to proliferation of the remnants of the medullary canal, neurenteric canal, and hind gut, in association with ectodermal and mesodermal inclusions.

Virchow, Ahlfeld, and Henning find it difficult to explain by the monogerminal theory some of the more complex teratomata, where an entire limb, an eye, a bronchus, vertebræ, or rudiments of intestine or liver may be demonstrated. These evidences have given birth to the bigerminal theory, which is, that such a tumor represents an incomplete monstrosity or twin, a parasite engrafted upon its autosite or host, the belief being that the existent fetus in its early career included the products of a fecundated ovum, a suppressed fetus, so called. Gross and microscopical evidence gives support to both of these theories.

The theory that these neoplasms are derived from a blastomere or impregnated pole-cell, which at an early age has been cast out of the complex, will explain the presence of the three necessary embryonic layers. This theory also has its advocates, but it is not generally accepted.

The hairy polyp of the throat and mouth cavities and the solid teratomata at the base of the skull may be explained by inclusions of ectoderm anlage. The teratoids of the vagina and cervix are probably due to inclusions of myotome or malformations of the Wolffian duct. Wilms believes those of the kidney due to proliferation of the remains of the Wolffian body.

The sacrococcygeal tumors of this group vary as greatly, individually and morphologically, as do the dermoids and teratoids elsewhere. They present a great diversity of tissue. From the simple dermoid structures they may present cysts containing or lined with ciliated, flat, or columnar epithelium, fibrillar connective tissue, muscle fiber, hair, teeth, nerve tissue, glands, skin, intestinal remnants, bone, and cartilege—rudimentary or special organs. Such growth Rindfleisch names "histological potpourri." Masse states that ventral sacral tumors are generally cystic; other observers, however, report enough solid tumors to cause an appreciable percentage of the whole.

A preponderance of one tissue may appear; e. g., when ganglionic cells or neuroglia are present, the tumor is presumably derived from the neurenteric canal or the filum terminale, the so-called neuro-epitheliomata of the older writers.

True dermoid cysts may be found forming a part of the more complex mass or next to tumors of mixed construction. Transitional forms of tumors are recorded from the simple congenital cysts to the larger and more complex dermoids, monogerminal in origin; from these on to the simpler mixed or teratoid tumors; from these, again, to the elaborately complex masses due to a bigerminal implantation, or true fetal parasites.

Practically all of these tumors are definitely encapsulated and arise usually from the pelvic connective tissue or have a slight attachment to the sacral perios-

Fig. 3. Area in large tumor showing nests of embryonic cells.

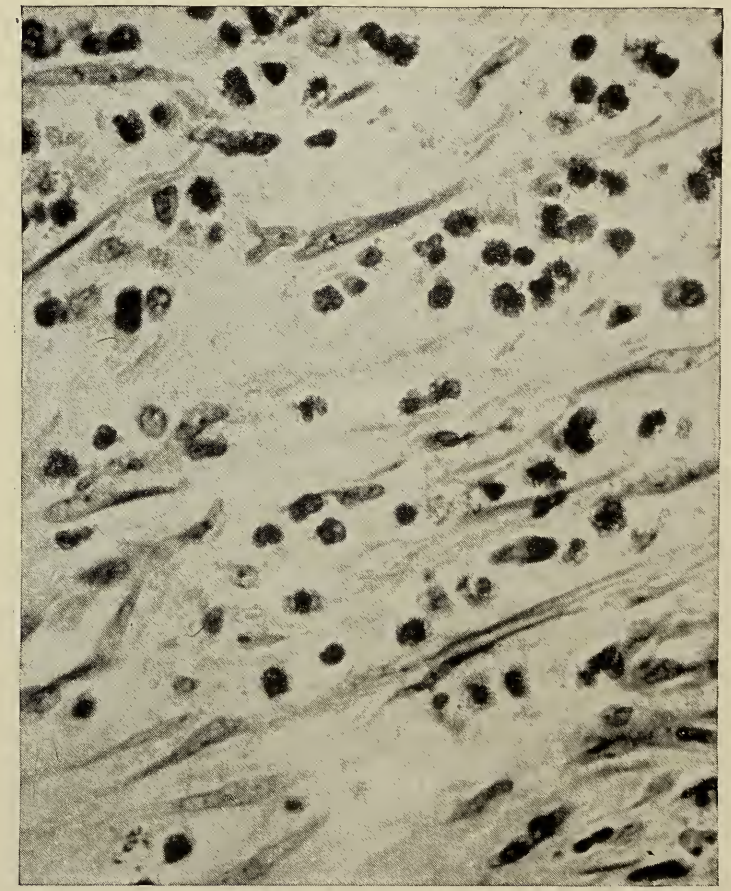

Fig. 4. Area in large tumor showing stroma infiltrated by eosinophiles.

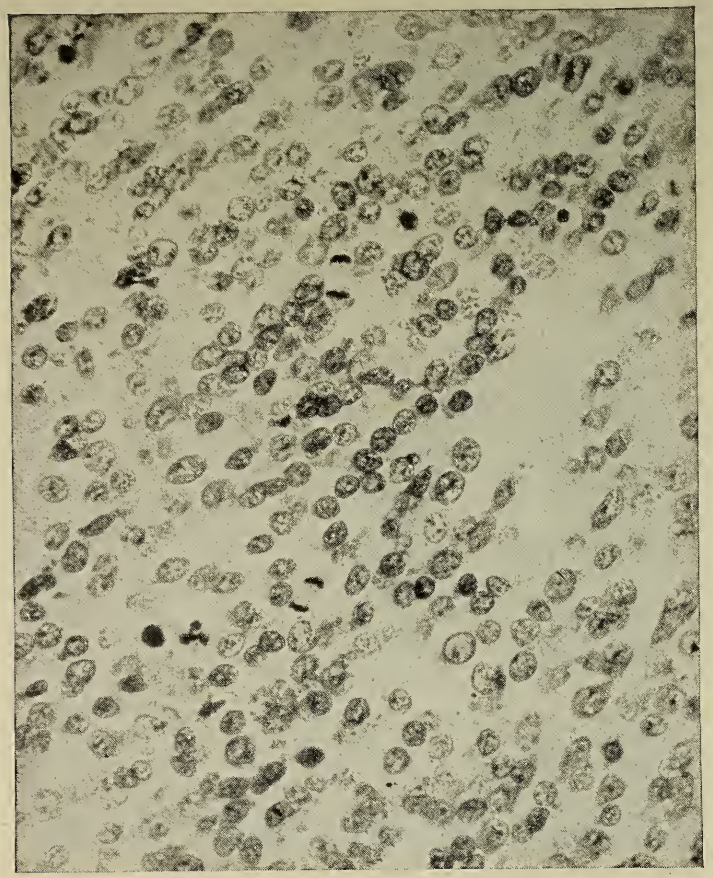

Fig. 5. High-power view of embryonic cells in large tumor. Note mitoses and variations in size of nuclei.

Fig. 6. Low-power view of small tumor, showing trabeculæ with collections of ganglion-like cells.

teum. They are seldom attached to bone, as are the majority of the dorsal sacral tumors; and our theories of their mode of origin offer a logical explanation for this.

Many of these tumors are not demonstrable save by vaginal or rectal examination; others may cause bulging of the perineal or coccygeal region. Again, large tumor masses may spring from a pedicle near the anterior sacral region and hang down from the body as a sacral appendage. Brennan and Choquette, Phillips, Pringle, and Davis report such cases. The growths nearer the skin are more often simple dermoids, while those higher in the pelvis are more complex and are much more rare.

These neoplasms are reported as rupturing into the vagina, bladder, or rectum, discharging their contents or pus into these adjacent organs. One case, reported by Keene and Coplin, opened spontaneously between the sacrum and coccyx; others are reported as rupturing through the perineum.

They may simulate uterine fibroids, ovarian cysts, intraligamentous cysts, or anterior spina bifida with meningocele, and generally give rise to symptoms from pressure on neighboring structures. These tumors have long been considered benign, but the cases of metastasis and local recurrence following removal recorded by Pringle and Tescher are significant.

Murphy calls our attention to the fact that they often contain mammary and testicular tissue, which is liable to undergo malignant change; and the reported, but very rare, chorio-epitheliomata in teratoids warrant the assumption that malignant transformation is not unusual. They are therefore on the border-line of malignancy and should be removed.

Reports of these tumors coincident with pregnancy are difficulty to find. Bernacki reported a dermoid cyst which interfered with labor, while Skulsch refers

to another dermoid cyst which he emptied through a perineal incision prior to labor.

Exostoses of the other pelvic bones interfering with labor are reported, and Bland-Sutton describes a large chondroma of the sacrum which prevented labor. In a careful perusal of the literature the writer can find no case like the following one, which prompted this paper:

Miss M. B., aged 16, was pregnant and at full term. She had been in active labor for twenty-four hours when I was called by the obstetrician, Dr. A. B. Cates, of Minneapolis. There had been no engagement of the child's head nor could it be made to engage. Bimanual examination revealed the soft anterior lip of the cervix, while the pelvis was filled by a hard, fixed, tense, non-elastic mass, which we believed to be a fibroid in the posterior cervical lip. This growth absolutely precluded delivery per vaginam, so Cesarean section was done after the method advocated by Kelly, where the uterus is delivered through the abdominal wound prior to its being opened.

We find that this procedure permits of an assistant grasping the uterine blood-vessels on either side, and absolutely controls hemorrhage when the uterus is opened. The child and placenta were removed, and, finding that manipulation back of the uterus would not dislodge the tumor, which was firmly fixed, we decided upon a panhysterectomy. It was only after this operation was started, the broad ligaments ligated and cut, and the anterior vaginal fornix opened, that we were able to feel the attenuated thinned posterior lip of the cervix. Then, and then only, were we able to recognize the tumor as a pelvic tumor, crowding close to and attached to the vagina and rectum. It could not have been removed by the abdominal route, so the hysterectomy was finished. The girl made a normal recovery, and her child lived.

The initial operation was done August 28, 1912, and on October 2, 1912, the removal of her tumor was undertaken, as we feared from its clinical aspect it might be sarcoma. Her hemoglobin was 60; she looked cachectic, and the tumor was growing rapidly. She was placed on her face in the reverse lithotomy position, and an incision was made from the upper part of the sacrum in the midline nearly down to the anus. The sacrum was chiseled across just below the second foramen at the level of the sacrosciatic notch and

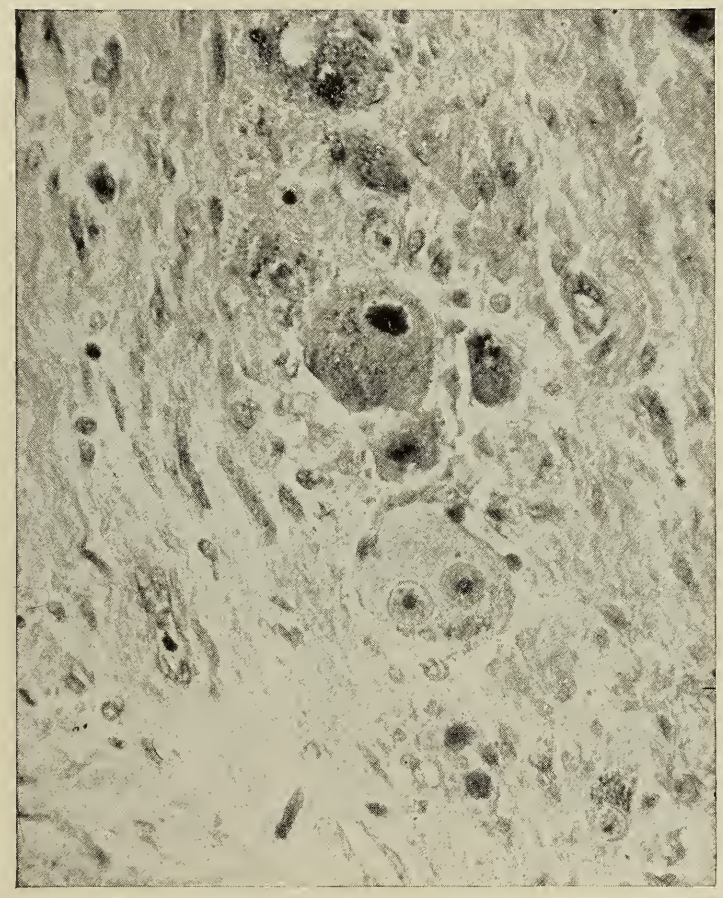

Fig. 7. High-power view of ganglion-like cells in small tumor. Note pigment and double nuclei.

removed, and with it the coccyx. A typical Mayo-Kraske exposure was done. The tumor had but a small attachment, one inch across, to the sacral periosteum, and was shelled out of the pelvis without much difficulty and with practically no hemorrhage. Some care was necessary to again remove the attached vagina and rectum. The girl made a normal recovery, and was discharged from the hospital November 10th.

Dr. H. E. Robertson, associate professor of pathology and bacteriology in the University of Minnesota, reports upon this tumor as follows:

Pathological Description.—Speciment consists of an oval mass measuring 9 cm. in length and 6 cm. in diameter, surrounded except at pedicle by a dense fibrous capsule. The external surface is irregularly lobulated and mottled by dusky-red areas showing through the capsule, which areas have a softer consistence than the whitish portions.

Cut surface shows the mass composed of distinct lobules of varying size. Some of these are distinctly hemorrhagic, the hemorrhage almost obscuring the grayish pulp. Other lobules are dense-white in color and often show a yellowish capsule or even have a yellowish tinge throughout. Small foci of calcification are present in these latter areas. Dense connective-tissue trabeculæ are plainly in evidence. Accompanying the main tumor is a smaller oval, very firm, grayish-white mass about 3.5 cm. in diameter. It sections with resistance of fibrous tissue, and its cut surface is smooth but marked by interlacing fibrous-like bands between homogeneous areas.

Microscopical Description.—Tissue fixed in Zenker and stained with Mallory's eosin methylene-blue. Sections of the main tumor show many areas containing masses of embryonic-like cells, showing vesicular nuclei, no definite cytoplasm, but with varying amounts of finely granular eosin-staining intercellular substance. In some cases the nuclei are closely packed together and in others more widely separated. No alveolar or other characteristic manner of grouping can be demonstrated. Many of them vary in size and intensity of staining properties, and mitotic figures are fairly numerous. These masses are surrounded by a loose connective-tissue reticulum, which in some places separates the cell-groups into distinct isolated areas with a large amount of intervening fibrillar tissue, while in other locations only delicate fine capillary-bearing strands are between the lobule-like collections of cells. In a few places the cells diffusely infiltrate the stroma. Hemorrhage into the stroma and cell-nests is irregularly distributed. The stroma shows long spindle-shaped cells with

occasional elongated bipolar filament-like processes of bastic-staining cytoplasm. A few are definitely multipolar. Capillaries are very numerous and thin-walled. Between these cells of the stroma there is a large amount of homogeneous eosin-staining substance in which a few fibrils are visible. Where the blood-vessels are most numerous, the tissue is infiltrated by large numbers of eosinophilicpolymorphonuclear leucocytes, and these leucocytes are also in excess in the blood-channels. Large areas of necrosis are common and a few foci of calcification are prominent.

Sections of the smaller mass show the tissue composed of dense fibrillar trabeculæ of various sizes, running in all directions and separated by a loose edematous stroma. In the larger of these trabeculæ are many large swollen rounded or oval cells, showing eccentrically placed nuclei with distinct nucleoli and eosin-staining cytoplasm. Some of these contain granules of golden-yellow pigment, and occasional rounded hyaline bodies are present. A few contain double nuclei and others show various stages of degeneration and cell death. Although no processes can be demonstrated, their morphological resemblance to nerve-ganglion cells is striking.

Conclusions.—The large tumor shows a loose, rapidly growing, connective-tissue stroma in which are nests of embryonic cells growing in syncytial-like masses and suggesting malignancy in their manner of growth and appearance. Areas of hemorrhage, necrosis, and calcareous degeneration demonstrate the atypical character of the tissue, its rapid development, and imperfect blood-supply.

The smaller tumor suggests a neuroma in its general character, although neuroglia fibrils have not been demonstrated.

That the tumor springs from some misplaced remnants of the terminal portions of the neural tube seems a logical conclusion. That portions of it have developed abnormal powers of proliferation and shown signs of malignant change, while other portions have, in an imperfect manner, reproduced the adult type of nerve tissue, is apparently the only explanation which accounts for these rather widely diversified tumor masses.

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DISCUSSION

Dr. E. S. Judd (Rochester, Minn.): Since Dr. Law called my attention to this interesting case some weeks ago, I have reviewed our own cases of retroperitoneal tumors and found in the list five that in some respects are similar to Dr. Law's case. I will give the history of each one of these cases. The five cases were all operated on, three by Dr. W. J. Mayo, one by Dr. C. H. Mayo, and one by myself.

Case 1.—The first patient, a woman, forty-four years of age, came to the hospital on account of hemorrhoids, constipation, and bleeding. A tumor the size of an orange could be felt per vaginam and rectum. It came to the midline in the pelvis and extended to the right side. Examination otherwise was negative. Wassermann was negative. Operation, November 21. A low abdominal midline incision was made. The retroperitoneal tumor apparently grew from the right wall of the sacrum about the second foramen, bulging out as a tumor the size of an orange under the periosteum and deep fascia, lying between the upper rectum and lower sigmoid and cervix, and completely filling that portion of the pelvis. The posterior peritoneum was separated, the floor of the pelvis opened, and the tumor enucleated. As it was enucleated, Dr. Mayo thought it resembled a hypernephroma. It was broken down and was removed in pieces. The pathologic report on the case (some three years ago) was that it was inflammatory tissue. There was an opening in the sacrum the size of the end of a finger near the third foramen, through which the pedicle of the tumor seemed to pass.

Case 2.—The second case was a woman, fifty-seven years of age, who came on account of rectal incontinence, hemorrhage,

and recent obstruction of the bowel. She also had radiating pains from the region of the sacrum and thigh. Examination showed a tumor posterior to the rectum. In this case a posterior incision (Kraske), was made and the lower end of the sacrum and all of the coccyx removed. A tumor the size of an apple was apparently a sarcoma of the anterior surface of the lower end of the sacrum and upper coccyx. The lower sacral vertebræ were involved. The tumor had perforated posteriorly, and a large amount of soft sarcomatous tissue had escaped into the hollow of the sacrum. The report of the pathologist showed it to be a giant-cell sarcoma. This patient was operated on June, 1912, and is still living.

Case 3.—The third case was a child two years of age, with a tumor at birth over the sacral region, the size of a fist, which had grown in proportion to the rest of the growth. At the operation a soft tumor was removed containing many cysts, which entirely surrounded the rectum, projected well out into the buttocks, and completely filled the hollow of the sacrum. The pathologic report was sacral teratoma.

Case 4.—The fourth patient, a woman, aged forty-three, came for consultation because of a tumor in the pelvis, discovered by her home physician six years before at the time of confinement. There were no loss in weight, no constipation; some pain before micturition. At the operation a large rounded pelvic tumor, situated low down, pushing the vagina, uterus, and rectum anteriorly against the symphysis, was removed. *X*-ray examinations showed no evidence of the bony growth. The tumor, the size of a child's head, was removed through the abdomen. It lay posterior to the rectum attached to the sacrum and coccyx. The pathological report showed it to be a degenerating myoma or sarcoma. The patient has been all right for over a year.

Case 5.—The fifth case was a man, thirty-three years of age, who gave a history of having been constipated for two years or more. He had had sudden, intense pain over the sacrum, which began two years previously. The large rounded mass was situated more to the left filling the hollow of the sacrum. Through a Kraske incision the tumor was removed with the entire coccyx and part of the sacrum. It proved to be a sarcoma, having its origin from the periosteum on the anterior surface of the sacrum. The pathologic report states it is a myxosarcoma. This was two and a half years ago. The patient had had over two hundred injections of Coley's serum up to about a year ago, but we have learned that now he has a recurrence. There is no doubt that the tumor was a sarcoma.

In looking these cases over Dr. Wilson called our attention to an article by Dr. Ewing on tumors of the testicle, which came out in the *Journal of Anatomy* a year or so ago. Dr. Ewing believes that a great many tumors of the testicle, diagnosed as sarcoma and carcinoma, are teratomas. Dr. Wilson reviewed 21 of our cases histologically and found that 19 of the number which had been reported as sarcoma and carcinoma, were teratomas. It occurred to me in talking with Dr. Wilson that these teratomas of the sacrum might be similar to those of the testicle. Dr. Ewing has called attention to the point that embryonic epithelial cells are found, as in the case of Dr. Law, and that these teratomas are malignant.

Dr. Jabez N. Jackson (Kansas City, Mo.): I enjoyed the paper of Dr. Law very much, and, knowing that these ventral tumors of the sacrum are rare, it is permissible to add one's individual experience.

I have had the opportunity to observe two of these cases of ventral tumor of the sacrum of two different types, one of which was exceedingly interesting from the fact that the woman was brought under observation primarily for the purpose of Cesarean section. She was a rather young woman (twenty-six years of age), and the presence of this tumor in behind the rectum was recognized by a country doctor, a young fellow, some time before pregnancy, who warned her at the time not to permit herself to become pregnant. The doctor was not called until she had been in labor forty-eight hours. He brought her to Kansas City at once for operation. There was a tumor of the ovary as large as a cocoanut, and with one finger in the rectum it went up in behind the sacrum. Through the vagina I could not palpate the cervix at all, so immediate Cesarean section was done, followed by hysterectomy. The tumor presented itself a little to the left of the rectum, and turned to the right slightly, as well as anteriorly. In splitting up the peritoneum the tumor was enucleated practically without any bleeding. As the doctor said, it is a characteristic of some of the cases, and after enucleation it showed no apparent point of attachment whatsoever, except when shelled out the whole anterior surface of the sacrum was revealed. It was enucleated without the presence of a pedicle or without definite knowledge as to its original point of origin. It was a single solitary cyst, which, on pathologic examination, was a simple cyst without the determination of what elements could have been the original focus.

The second case that came under my observation was a man, forty-five years of age, with general symptoms which mainly

attracted attention; that is, pain in the lower pelvis, particularly about the rectum on defecation, and some pains which radiated down the limb until the doctor thought there might be some trouble about the hip-joint. On rectal examination a tumor could be outlined near the lower end of the rectum about the size of a small orange, and in operating on this I made an incision laterally, but, upon entering the tumor, found fluid that was cystic and purulent. It seemed as though some inflammatory secondary condition had taken place, and the wall was so friable it was impossible to make any enucleation whatsoever. We removed certain portions of it and packed for drainage.

These cases are very rare, I am sure from a perusal of the literature at the time this case came under my observation, and their rarity is my excuse for presenting these individual cases.

Dr. Law (closing the discussion): This tumor presented so many different characteristics that the pathologists were nonplussed. I remember taking sections to Rochester and having them examined. Dr. Wilson suggested that most of the cells were embryonic cells, but he said the tumor, in all probability, would recur, and, when it did recur and went on to development, it would be a sarcoma. In talking with the Mayo staff I learned they had had a number of cases of ventral tumors of the sacrum, and Dr. Judd kindly looked them up. In the five cases just reported by him, it is interesting to note that in only one instance was the tumor a dermoid. Three of them were sarcoma, and one an inflammatory tumor. Anyone who is familiar with the tremendous amount of clinical material which goes through their hands at Rochester will appreciate the significance of the fact that in only one instance was the tumor a dermoid. It forcibly emphasizes the rarity of these growths.

Dr. Jackson's two cases are interesting from the fact, as I mentioned in my paper, that most of these tumors are cystic. In fact, in a study of the literature almost all of the cases reported were cystic, and I found only two instances where the mixed anterior tumors of the sacrum are reported, and no case reported of this character in conjunction with pregnancy.

COEXISTING INFECTION AND SARCOMA OF THE THYROID, WITH THE REPORT OF A CASE

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Sarcoma of the thyroid is a rare disease, and sarcoma of the thyroid complicated by infection is still more rare.

Müller and Speese[1] found only 11 cases of sarcoma recorded in the literature of this country prior to June, 1906. Only 11 per cent of these cases occurred in patients under 30. In 53 per cent of malignant disease of the thyroid,—sarcoma and carcinoma,—they found a previous history of goiter, and in more than half the cases this history extended over a period of more than ten years. Two-thirds of the cases were in women, and 53 per cent of the cases occurred between the ages of 40 and 60. They found adenocarcinoma and round and spindle-cell sarcoma the most frequent types. Metastasis involves the lungs and bones most commonly and, of the latter, especially the skull and inferior maxilla.

They report that carcinoma of the thyroid invades distant parts by way of the blood-vessels, and sarcoma through the lymphatics with greater frequency than is the general rule. Malignant goiter caused death in 70 per cent of the cases reported by them. I have made quite an extended, but not exhaustive, search of the literature, but have succeeded in finding only

four cases beside my own of sarcoma of the thyroid reported since 1906.

E. A. Vanderveer, of Albany, reported[2] a case of sarcoma of the thyroid in 1907 occurring in a male aged 70. He gave a history of goiter extending over a period of six years. The growth was the size of two oranges, symmetrical, movable, and painless. A diagnosis of cyst of the right lobe was made, and the growth was removed under local anesthesia. There were many adhesions and much hemorrhage. The man recovered and left the hospital at the end of twenty-one days with the wound healed. The laboratory report was fibrosarcoma.

Nasette[3] reports a case of sarcoma of the thyroid as a recurrence of carcinoma, but I could not obtain even an abstract of his article for the present paper.

Kocher says that sarcoma (aside from endothelioma) and carcinoma may occur in the thyroid at the same time, but that the combination is rare. He further says that it is remarkable that this combination has apparently been observed beyond the possibility of a doubt only in cases in which there was some doubt that the epithelial tumor in the thyroid was primary.

Chavanez and Pierre Nadal[4] report a case of tumor of the thyroid in a woman 64 years old, which was removed after it had existed a "long time," and which, upon microscopic examination, was found to contain sarcomatous areas, together with osteoid, chondroid, and ordinary connective tissue areas.

W. H. Snyder[5], of Kalamazoo, reports a case of sarcoma of the mediastinum which invaded the thyroid. The patient was a male, aged 37. The symptoms,—cough, difficult breathing, and hoarseness,—antedated the appearance of the thyroid tumor three months. The tumor was very hard. One month

after the first appearance of the thyroid tumor it was removed by operation. The cervical lymph-glands were also involved. The patient died forty-one days after the operation from increasing obstruction to breathing and weakness. At the autopsy the tumor was found adhering to all neighboring organs except the left lung. No metastases were found outside of the mediastinum and neck. The specimen was submitted to Dr. A. S. Warthin, of Ann Arbor, who reported as follows:

"The growth is a round-celled sarcoma, the majority of the cells being of the large round-cell type. There is no evidence of any leukemic condition in the blood-vessels, and, unless the process was localized in the lymph-nodes as a primary condition, the tumor should be classed with the simple sarcomata. The thyroid is invaded, and there is no subpleural metastasis invading the lung tissue. If the lymph-nodes were the seat of the tumor process, the condition would fall into the group of aleukemic, large-celled lymphosarcoma."

My own case is as follows:

B., male, age 12, admitted to Hope Hospital, September 5, 1912. Family history, good. Never been ill until present trouble came on ten weeks ago. At this time he noticed a small lump on the front of the neck just above the end of the sternum. This has been growing and there also appeared lately a small lump above and to the left of the original.

Examination showed a well-nourished blonde lad with a goiter the size of a lemon, which seemed to involve equally all parts of the gland. The skin was not discolored or attached to the goiter, which was very firm, not nodular, slightly tender, and not painful. Above and to the left of the goiter there was a somewhat tender lymph-gland as large as the end of

my thumb. This gland was freely movable. Urine was negative. The blood showed 20,200 white cells, with 74 per cent polyneuclears. The temperature ranged from normal to 100.6° F. There were no symptoms of hyperthyroidism. At times there was some choking sensation. Slight headache. Appetite poor. Bowels regular. It was thought that the trouble might be entirely inflammatory, although the probability of malignancy was recognized. Operation was advised and accepted.

On September 10th, five days after admission, the usual collar incision was made under ether. It was found impossible to separate the muscles and fascia from the surface of the goiter, save by cutting, on account of the dense adhesions. The gland structure was yellowish-gray in color, not vascular, firm, and somewhat friable. That portion of the gland immediately overlying the trachea was removed, a drain inserted, and the skin wound closed up to the drain. The wound healed promptly, and the boy left the hospital sixteen days after the operation. I here show you the portion of the growth removed. Dr. Rhamy's report is as follows: "After a careful study of the sections from the thyroid of L. B., I find that beneath a very marked inflammatory infiltration there is undoubtedly a spindle-celled sarcoma. This shows plainly in areas where there is little leucocytic infiltration. In the greater part of the growth, however, the leucocytic infiltration is so intense that the sarcomatous growth can be made out only in the light of the other fields, which do show it plainly. Nowhere in the growth can I find any recognizable thyroid structure."

Figs. 1 and 2 illustrate this description. It is important to say here that the report on the frozen section made at time of operation was that the trouble was inflammatory.

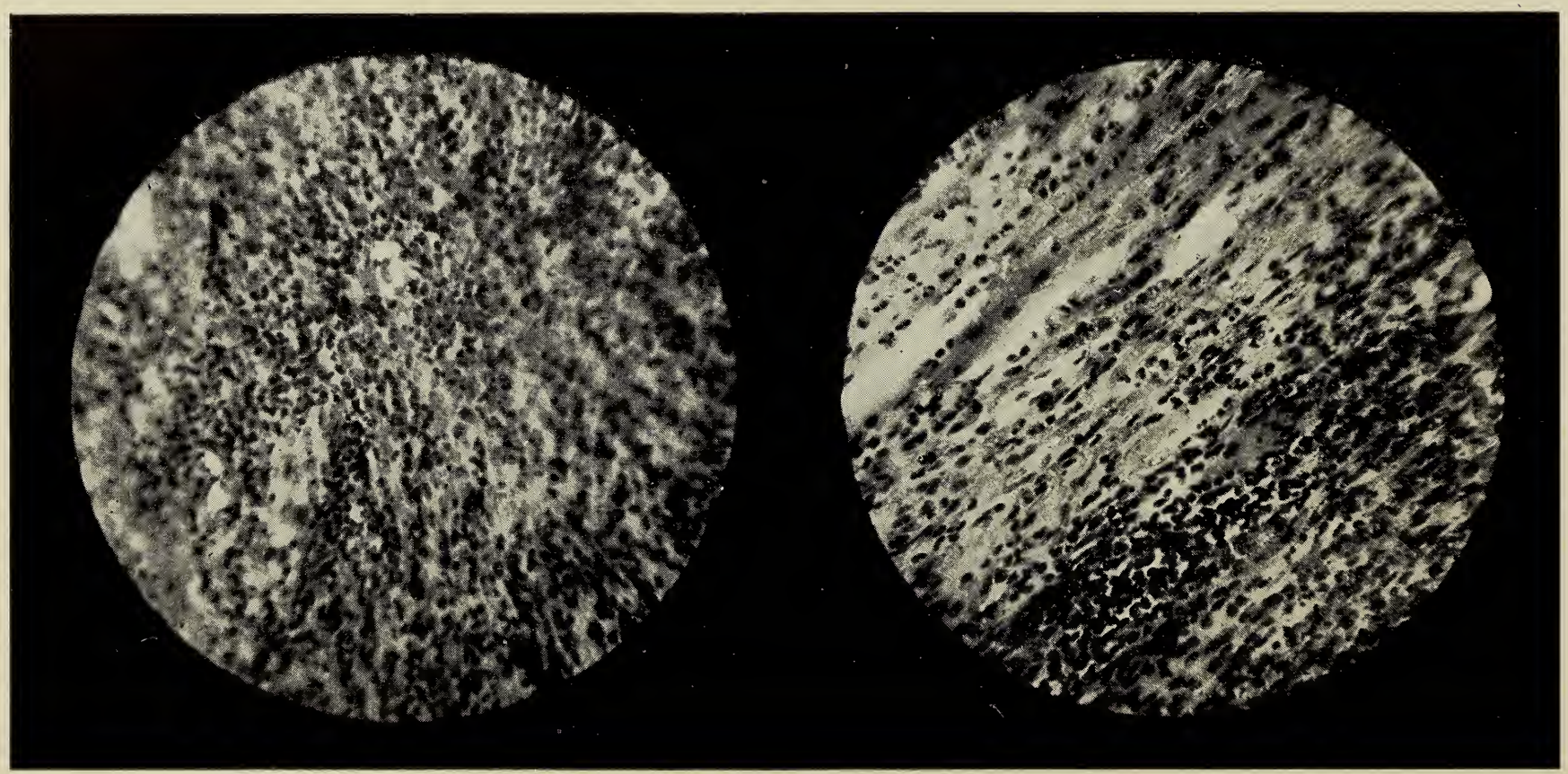

Fig. 1. Author's case, showing leucocytic infiltration and occasional giant cell. Note the absence of typhoid tissue. Rhamy ⅙ obj.

Fig. 2. Author's case, showing spindle cells with leucocytic infiltration. Note the absence of gland tissue. Rhamy ⅙ obj.

He also states in a subsequent communication that both cultures and smears demonstrated a staphylococcic infection. The report from the Columbus Laboratory is as follows:

"Specimen is made up of embryonal connective tissue that shows little or no tendency to form adult connective tissue. There are a few foci of necrosis. It is a small round-celled sarcoma."

I tried to get a late report from the patient, but failed, so I cannot say whether he is still alive or not.

According to Müller and Speese, the round and spindle-celled are the most frequent types of sarcoma found in the thyroid. Of the cases collected for this paper there are two in which the type is noted, one fibrosarcoma and one large round-celled sarcoma. In my own case the two pathologists who examined the specimen differed somewhat, the one pronouncing it to be a small round-celled sarcoma and the other classifying it as belonging to the spindle-celled variety. According to Kocher[6] the spindle-celled type is the most common and the large round-celled comes next, while the small round-celled variety, he says, is rare. Spindle-celled sarcomata may be converted into bone or cartilage. The case of Chavanez and Pierre Nadal, reported above, illustrates this. This author (Kocher) also says the round-celled variety often resembles lymphosarcoma so closely as to make the differentiation difficult. It will be remembered that Warthin, in his report on Snyder's case, as noted above, suggests that that may be a lymphosarcoma of the large round-celled type. The complete destruction of the gland-tissue by the neoplasm is quite common. It will be remembered that in my case Dr. Rhamy was unable to find "recognizable thyroid structure."

The bones are frequent seats of metastatic growths originating from malignant disease of the thyroid.

Especially are the sternum, ribs and scapulæ apt to be attacked in this way in sarcoma of the thyroid.

Invasion of distant structures by way of the lymphatics from sarcoma originating in the thyroid seems much more common than is the usual rule concerning metastasis in sarcoma. Had I been better acquainted with this fact I should not have allowed the lymphatic involvement in my case to weigh so heavily as I did in favor of a diagnosis of thyroiditis. Concerning the combination of infection with sarcoma in the thyroid, I could find no case reported. In view of the frequency of infection in malignant growths, generally, however, it is fair to presume that the combination is not rare. There is very little to say concerning the diagnosis of malignant thyroid disease, but it is important, perhaps, to emphasize two or three points:

First. I have a feeling that with malignancy in enlarged thyroids, as with malignancy in large prostates, the condition is more frequent than it is generally thought to be. This feeling is a result of the systematic and careful examination of all goiters removed by me for the last year or more. As yet I am not able to report in detail.

Second. Comparatively sudden and rapid increase in size in an old goiter should arouse suspicion of malignancy, especially if this rapid growth is accompanied by pain.

Third. Hemorrhage into an old goiter should raise the question of malignancy.

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6. Keen's Surgery, vol. iii, p. 388.

Dr. Charles H. Mayo (Rochester, Minn.): Dr. Porter's paper deals with an interesting and rare condition. In this region of the neck, where the lymphatics are free, we may find sarcomas and teratomas, just as we would expect to find carcinomas. During the past year, in somewhat over 1,200 operations on the thyroid in our clininc, there were eleven cases of carcinoma, one of sarcoma, and one of syphilitic tumor of the thyroid, which was removed. In certain groups of cases when we have difficulty in differentiating between the conditions it would be well to make a Wassermann reaction. These cases are interesting from the diagnostic standpoint. As Dr. Porter says, the rapid growth, the irregular character and the density of the tumor of long standing are indicative of malignant degeneration. Early in the history of sarcoma the tumor is usually rounded and quite dense. One will occasionally find encapsulated adenoma giving a history of recent hemorrhage just as hard and just as dense. Upon exposing the tumor, as a rule, the conditions as described are presented. That is, it has gone through the capsule and involved the muscles so that it is almost impossible to separate them. In such cases a cure is rarely affected with any type of operation since the disease is no longer confined to the original point of origin.

Dr. L. L. McArthur (Chicago): Dr. Halstead of Chicago operated on a case of sarcoma of the thyroid a year and a half ago, in which there was primary union, but recurrence took place. The patient subsequently came in the Michael Reese Hospital after having been in St. Luke's Hospital with an infective process, which I thought was actinomycosis, with sinuses leading into the mediastinum. Not being able to demonstrate the organism, we put it down as a simple inflammatory affair. The post-mortem, which followed after three or four months of long-continued suppuration and spreading, showed it to be a sarcoma associated with an infective process of the thyroid, having recurred in the cicatrix and stump of the thyroid, which Dr. Halstead had operated on.

A CONSIDERATION OF FINAL RESULTS IN HYPOPHYSEAL SURGERY, WITH THE REPORT OF A CASE

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While the final word has not by any means been said as to the technic involved in the surgical treatment of diseases of the hypophysis, the general line of procedure at least has been determined, and we have progressed far enough to demonstrate the propriety and feasibility of surgical intervention in certain cases.

A sufficient number of cases have now been subjected to operation for us to begin to consider carefully the results of these operations, not alone as to life, but as to the alleviation of the symptoms and the influence upon the course of the disease after the lapse of a considerable period to the end that we may know what may be expected as a final result, and may not rush our patients into ill-advised orperations before the possibilities are known. Because a patient survives an operation is no proof that the operation was successful. The following case is, therefore, reported:

CASE.—Typical Frölich type of hypophyseal disease, sufering with marked signs of intracranial pressure. Operated upon two years ago by infranasal route; cyst evacuated; recovery; result of feeding pituitary gland over two years.

History on entrance to hospital.—Stanley W., age 18; male; white; native of United States; admitted to Wesley Hospital

February 20, 1911, under diagnosis of hypophyseal disease, made by Dr. Hugh T. Patrick.

Complaint.—Severe frontal and temporal headache; impaired vision, patient unable to recognize objects—only light and darkness; photophobia; persistent vomiting.

Family History.—No history of tuberculosis or malignancy.

Habits.—Appetite fair; bowels, regular.

Previous Illness.—Measles, whooping-cough, and chicken-pox in childhood; typhoid at eight, being sick about three months; suffered with enuresis till he was 16; has been weakly ever since he had typhoid.

Present Illness.—The present trouble dates from his 14th year. Though mentally bright and well developed, he lacked physical development and vigor. His appearance was anemic. His voice did not show the normal change which takes place about puberty, and he remained sexually undeveloped. He began to have frequent attacks of sick headaches with which he had considerable gastric distress, nausea, and vomiting. During his fifteenth year he began to notice that his eyesight was failing and obtained considerable relief by using glasses, but on account of more or less photophobia he wore smoked glasses part of the time. Under these conditions he continued his work as clerk in comparative comfort until about a month ago, when his eyes suddenly failed him. He had aching pains in the eyeballs. At times his pupils would dilate markedly, especially when the pain was bad. Photophobia and impaired vision became more marked. With this he developed an acute and spasmodic frontal and temporal headache. The pains were sharp and shooting and frequently radiated to the occipital region. On February 18th he had his first attack of vomiting, which he attributes to the onset of a paroxysm of headache. These attacks became more severe, so that for three days he has had continued headache and vomiting and has eaten nothing.

Examination.—General appearance is that of a boy of twelve years, slightly anemic, full growth of nails, and hair on head. General conformation: adiposity above the average, but not excessive, due possibly to prolonged vomiting (acetone marked in urine); no excessive development of breasts; no growth of hair on body or pubic region, except fine hairs invisible to ordinary inspection; back, normal; ears, normal; teeth, normal; nose, normal. Dr. Goetz of Naperville says that he had a third development of teeth, 5 or 6 of the second group being cast off at about 12 and a third group appearing. Right

eye slightly divergent, pupils slightly large, and they have been so while patient has been in hospital subsequent to the operation; patient states that they are always so during his attacks of pain, but that they may be smaller between attacks. The testicles are both present, but are very small. The penis also is very small.

The examination of the thyroid, lungs, heart, and abdomen is negative; height, 145 cm. Patient says his height was 4 ft. 9 in. (i. e., 145 cm.) a year ago.

	Exam. at Hosp. Feb. 20, 1912.	Examination, Oct. 27, 1911.
Circumference of head, glabella to oc. pt.	52 cm.	52 cm.
Angle of jaw closed to bregma	60	
Circumference of chest, expanded, 73 in.	67.5	exp. 67 in. 74
Circumference around shoulders	77	81
Circumference of humerus at middle	18	19
Circumference of forearm at middle	15.5	16
Length of arm, acromion to tip of middle finger	65	65
Length of elbow to tip of middle finger	39	39
Circumference of thigh at middle	35	39
Circumference of hip through trochanter	70	73.5
Circumference of leg at middle	26	27
Length of leg, ant. sup. spine to int. malleolus	79 cm.	78.5

Operation.—February 21, 1911, by Dr. Kanavel, assisted by Drs. Wolfer, J. Gordon Wilson, B. C. Cushway, and H. M. Richter. Patient had been given urotropin at home for one week previous to entering hospital. Just before being anesthetized, the patient's nose was packed with strips of gauze saturated with 5 per cent cocaine, and as soon as the patient was anesthetized these were removed and the nose packed with adrenalin gauze, full strength. The anesthetic (ether) was now changd from oral to rectal, and this was given throughout the operation under the supervision of Dr. J. F. Churchill, with complete satisfaction. An incision was made close under the nose and on the nasal crease at the side, and the technic carried out exactly as described in previous contributions.[1] The middle and inferior turbinates were removed. There was some difficulty in locating the sphenoidal ostea, so that it was necessary to chisel through bone directly. No evidence of a sphenoid cell was found at any time. The dura was exposed and a bulging pearl mass about three-fourths inch in diameter was exposed. There was no marked pulsation until partial evacuation of the cyst had been made. An attempt was made to aspirate the tumor since a diagnosis of a cyst had

1. Jour. of the A. M. A., Nov. 20. 1909, and Surg. Gynec., and Obst., April, 1910.

been made by Dr. Kanavel. The aspirator did not work, however, so that it was concluded that the diagnosis of a cyst was incorrect and with a scalpel an incision was made the length of the exposure through the dura. Immediately the underlying tissue bulged through and a chocolate colored fluid escaped, and fully an ounce ran out, of which about one-half ounce was saved and given to Dr. S. A. Mathews for examination, who reported that the fluid contained a pressor substance. He injected it into a dog and obtained an increase of 20 mm. pressure of mercury which persisted for half an hour. Dr. Mathews draws attention to the fact that cystic fluids generally cause a fall in blood-pressure.

The cyst was curetted thoroughly and the contents given to Dr. Zeit for examination. These consisted of a granular detritus. An attempt was made to secure the cyst-wall, but it was unavailing. A strip of iodoform gauze was inserted into the cyst and the end brought out through the nose. The nose was packed with strips of gauze, the posterior nasal plug, which had been inserted before the operation and which had effectually prevented the blood from entering the wound, was left in. The nose was sutured in position, the septum being brought to the middle line and held in place by the ends of the packing-strips which were brought out through the nose. (Fig. 3.)

The patient left the table in good condition. Subsequently he was given olive oil by rectum to allay the irritation of the ether. Normal salt was given, subcutaneously, as a stimulant and to help remove the ether from the system.

Subsequent History.—The first strips of gauze were removed at the end of 36 hours, more at the end of 48 hours, and the final strip leading into the cyst was removed in 56 hours with the posterior nasal plug.

Following the operation, the patient complained of headache, and vomited rather constantly for 48 hours, when the vomiting ceased completely. The cause of the vomiting may be attributed to one of two things, either a retention in the cyst or post-anesthetic vomiting. I am rather inclined to look upon the former as the cause, since it came on so suddenly and after vomiting the patient would have complete relief until the next attack. This view is further supported by the fact that on removing the strip of gauze at the end of 48 hours, a gush of at least one-half ounce of fluid flowed down, followed by complete cessation of the vomiting, except when it could be directly traced to other causes, such as secretion dropping back into the mouth.

His polyuria returned after about ten days. Then it was noted that he had a marked acetonuria, which yielded largely

under levulose and carbohydrate treatment. The polyuria also subsided rapidly.

An infection developed in the face, suggestive of an erysipelas, and subsided in a week. No other outward symptoms appeared. His eyesight began an immediate improvement. His headache disappeared on the third day, and has not returned.

I am indebted to Dr. Cassius C. Wescott for the two charts denoting the changes in the fields after the operation, and for the following notes upon the "eye-findings":

> S. W., American, male, was seen first, at the age of 15 years, on October 8, 1908, complaining of dimness of vision. He also complained of pain, frontal and temporal, radiating to the occiput. Photophobia and poor vision were helped in 1904 by securing glasses which he wore for nine months.
>
> At the time of examination (October, 1908) he stated that his vision had become dim in the right eye six days previously, and after two days had suddenly failed entirely. R. V., hand movements; L. V., 6-12; the right pupil exhibited no direct light reflex. The maculæ were both very granular, and the retinæ of the "shotsilk" type. No lesion was found to account for the poor vision, which was regarded as a hysterical manifestation. This opinion was strengthened by the improvement in the right vision to 6-15 in twelve days, and 6-9 plus in one week more, with practically no treatment except strong suggestion of recovery. Refraction under atropine cycloplegia on October 28th revealed the following:
>
> R. E. + 1.75 Sph. 0.25 Cyl. Ax. 75°=6/9+2.
> L. E. + 1.50 Sph. 0.50 Cyl. Ax. 80°=6/9—2.
>
> He was given this correction less +0.50 Sph.
>
> Nothing further was heard from the boy until February 3, 1911, when he reported with the complaint of severe sick headache, which he attributed to a fall four weeks previously. He had slipped on an icy pavement and struck the back of his head. He was stunned, but did not lose consciousness, and succeeded in walking to his home, a distance of several city blocks, without help. He was very positive that some steady failure of vision had been going on for a time previous to the accident, but the failure proceeded more rapidly thereafter until one week before reporting, when a sudden decrease in vision occurred, more manifest in the right eye, which began to turn outwards.
>
> Upon examination, we found the left eye to fix while the right diverged moderately. Ocular movements were full in all directions though, with both eyes open, attempted convergence was practically *nil* with the left eye. The conjunctivæ were unduly pale, the corneæ clear, the pupils 3.5 mm. and round (the right possibly very slightly larger than the left). There was very pronounced hip-

pus, especially on the right; and occasionally, when bright light was thrown into the right eye, the pupil would first dilate widely and then exhibit hippus. Both pupils reacted promptly to bright light and to convergence; moderate light caused no contraction of either pupil if the other was shaded, but caused contraction of both pupils when both were simultaneously illuminated.

Intra-ocular examination showed the media to be clear, both nerve-heads gray, the right being decidedly more atrophic than the left, with a small pigment heap at the upper temporal border. The disc edges were well defined, and there was no evidence of previous neuritis. The arteries were slightly contracted and tortuous, but the vessels and the central area of the retinæ appeared more nearly normal than one would suppose with such atrophic discs. The peripheral parts of both retinæ were thinned, the choroidal circulation showing very plainly. R. V., 6/45; L. V., 6/15, not improved by correction. The fields of vision, as seen in Fig. 1, show in the left eye a marked contraction in the temporal half; and in the right only a small paracentral area (temporal) preserved, with a crescent-shaped area of very vague perception of the moving object in the nasal field.

On February 10, 1912, after operation, he was comfortable and the left vision still 6/9. On March 7th he complained of blurring in the left nasal field. The left disc was found to be very pale, almost the color of the right disc.

Rhinoscopic examination revealed no mass projecting from the sphenoid, but only an exuberant mucous membrane. The field of vision of the left eye shows some temporal contraction as compared with the fields taken on March 30, 1911, but a wider field than that of February 3, 1911 (before operation). He was given strychnia, gr. 1/40, t. i. d. On April 2, 1912, L. V. 6/9 2. Strychnia was increased to gr. 1/30, t. i. d.

On October 15, 1912, the patient called at our request. The left vision is 6/9 2. He is feeling well and working with comfort. The fields (Fig. 2) show in the left a widening on the temporal side exceeding the field taken in March, 1912, and almost equal to the field of March, 1911, which is the best recorded. In the right eye there is a decided failure since March, 1911, the form field being only vague and limited to the nasal side not reaching fixation and being wanting in the part of the upper and lower nasal quadrants. The right vision is fingers at 8 inches instead of 1.45, as noted in March, 1911.

Following the operation the patient was given 8 grams of the anterior lobe pituitary gland for six months. No improvement was noted, and it was therefore deemed advisable to change to the whole gland, which was done. At the end of a year, he was re-admitted to the hospital and an attempt made to determine the sugar-tolerance after Cushing's method. With 120 grams of levulose we received no sugar reaction up to 30 grains of gland per

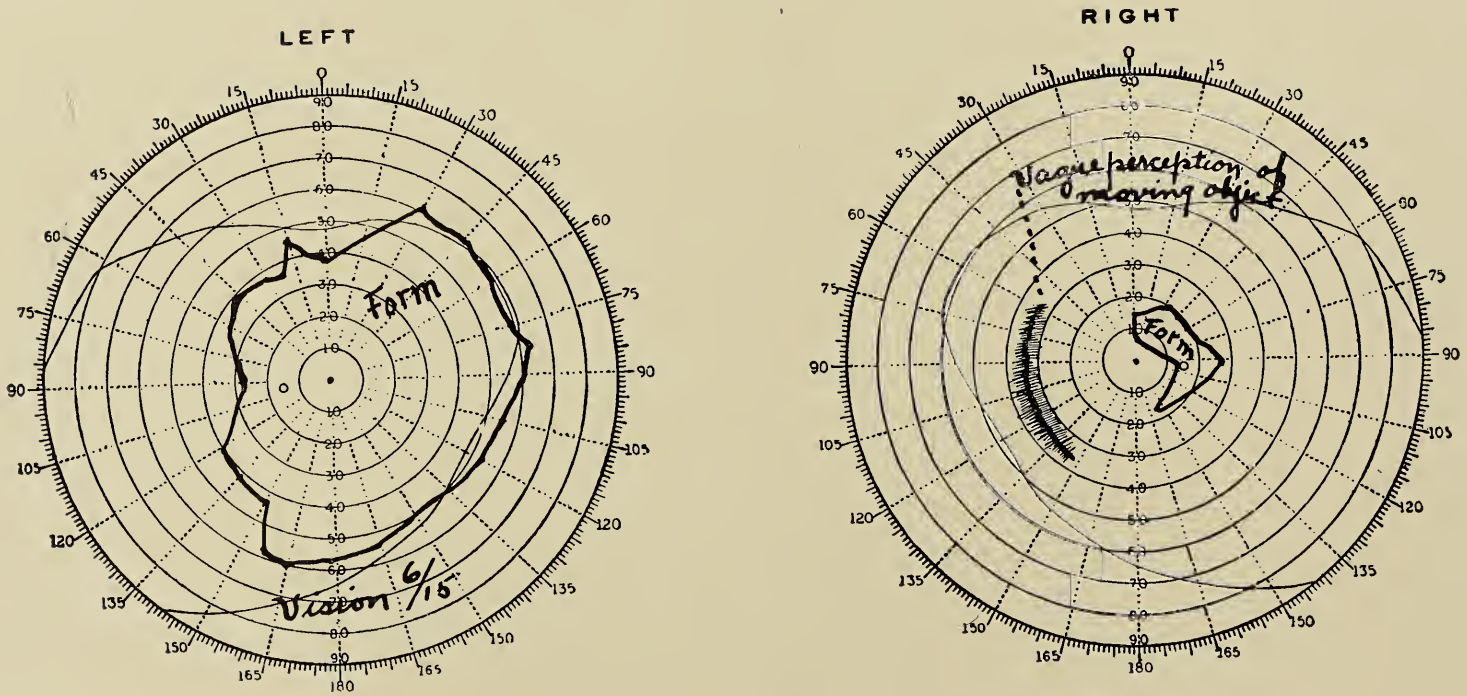

Fig. 1. Eye findings (Dr. Wescott) February 3, 1911, eighteen days before operation.

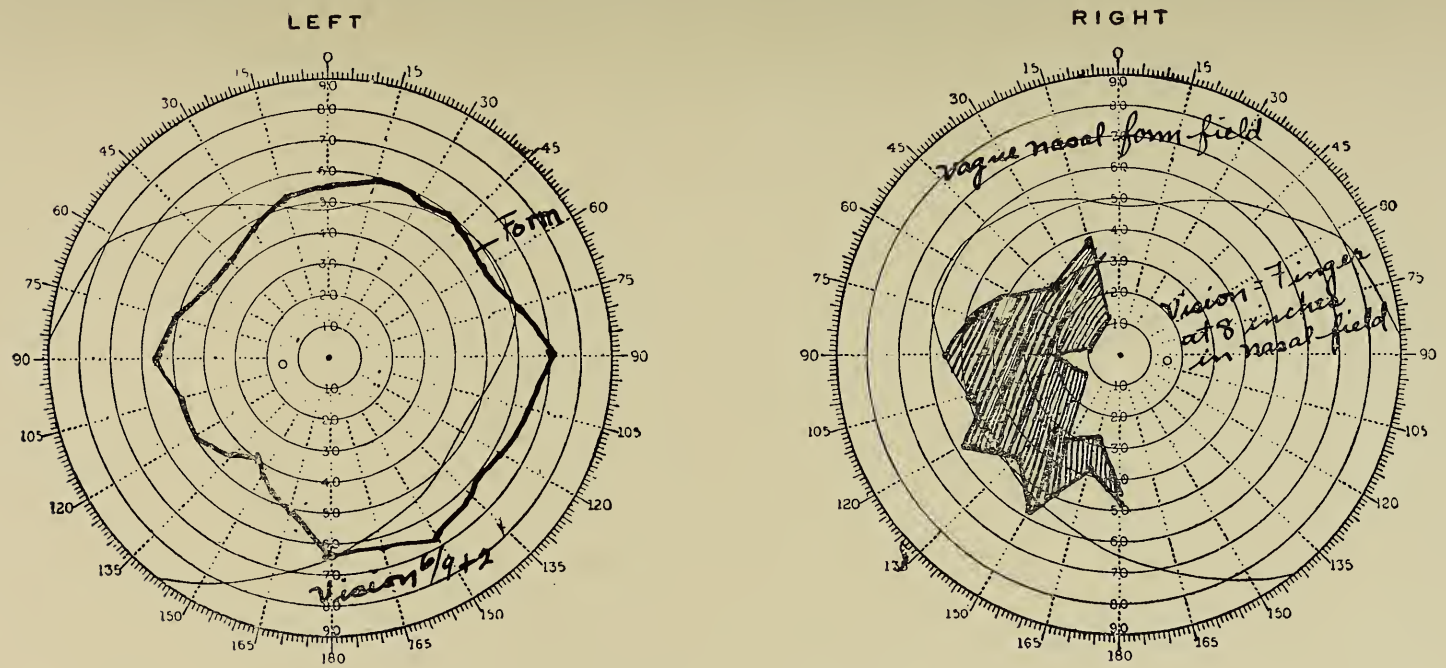

Fig. 2. Eye findings (Dr. Wescott), October 15, 1912, twenty months after operation.

day. The patient refused to remain longer in the hospital since he is in active business. We have therefore continued with 30 grains daily since, with the result that there has been a marked growth of hair upon the arms and legs, and a slight growth upon the face, pubes, and axillæ. There has been no return of the excessive adiposity. On the other hand, there has been no bony growth, his height has not changed, and no sexual change has occurred.

Summary.—The patient was suffering from a cyst of the hypophysis, which had caused primarily the type of disease known as Frölich's syndrome, upon which had been superimposed an acute attack of brain-pressure, with vomiting, headache, and complete blindness, due, doubtless, to a hemorrhage into the cyst. The operation performed by the infranasal route, already described and used by myself, was successful in evacuating the cyst. The walls of the cyst were curetted, and the cyst packed. The patient recovered and noted an immediate restoration of sight, practically complete in one eye and slight in the other. Headache and all signs of brain-pressure disappeared, and this freedom from difficulty has persisted for over two years. Meanwhile he has been fed upon varying doses of pituitary extract; at first, of the anterior lobe, and later of the whole gland in doses varying from 12 to 30 grains daily. There has been a distinct growth of hair upon the extremities and slight growth upon the face, pubes, and axillæ. He has lost his excessive adiposity. There has been no growth in height, nor any development of sexual functions, although his general appearance is tending toward that of the male. He is bright and capable of carrying on his work in a responsible position. (Figs. 4 and 5.)

A general review of the results reported to date by Cushing, Hirsh, von Eiselsberg, Hochenegg, A. E. Halstead, and others may be summarized briefly as follows:

Prognosis as to Life.—Of 45 cases operated upon by the supranasal route, 17 have died, a mortality of

37.8 per cent. Von Eiselsberg's mortality in the 12 cases was 25 per cent. The infranasal route, including the submucous modification, excluding Hirsch's cases, has given a mortality of about 21 per cent (36 cases, 8 deaths). I myself have operated upon 3 cases with 1 death. Cushing's mortality has been about 13 per cent, while Hirsch in 26 cases has had 3 deaths, a mortality of 11.5 per cent. Such figures are not conclusive by any means, but they do indicate that the mortality-rate is rapidly falling as our experience grows, and that soon we may be justified in operating upon those cases which fall into the elective group and not confine ourselves, as at present, to the operations of necessity.

Prognosis as to Ultimate Results.—What, in various types of disease, may be looked for as the ultimate result? While it is too early to prognosticate this, a review of the literature, as well as our experience, would seem to indicate the following:

Cysts may be operated upon with particularly good results as to local pressure. That they may recur, the experience of Horsley demonstrates. That they often do not, is proved by my experience in the case reported, as well as that of von Eiselsberg, who reports two cases well two years after the operation. Hirsch has operated upon five cysts, and he states that they have good chances for amelioration of symptoms. The eye-signs have been uniformly bettered, unless complete nerve-atrophy was present. The excessive adiposity has generally been lessened if the operative procedures have been supplemented by whole-gland feeding. The sexual development has been little affected, although von Eiselsberg reports slight change in one case. Marked change in growth has not as yet been produced, even with gland-feeding. The growth of hair and a tendency to change

from the female to the male type, has followed frequently if gland-feeding was instituted.

Owing to the fact that acute pressure-symptoms in these cases are due often to hemorrhage into a cyst, an absolutely bad prognosis cannot be given in those cases in which operation is refused, since it has occurred that the hemorrhage has been absorbed, and the evidence of acute pressure has disappeared. In these cases, and others not operated upon, whole-gland feeding may have some effect on the symptoms mentioned above, but certainly not as marked as in those cases operated upon and treated by subsequent feeding.

The results in operations upon acromegalies are not so definite, due, possibly, to the fact that operation has been performed after hyperpituitarism has produced its results and not at the inception of the disease.

There has generally been a recession of the swelling of the soft parts, but no effect upon the bones. Menstruation has been restored in several cases (Hochenegg and von Eiselsberg), and Cushing reports the return of male sexual function one year after operation, which was supplemented by gland-feeding. The eye-sight when involved has been favorably affected. There has not been a marked loss of weight, even with feeding. It is to be noted that these cases of acromegaly, which have gone over into the state of hypopituitarism with mental symptoms seem to be markedly benefited by feeding without operation. (Cushing). Sufficient observations have not accumulated to give us any data as to the effect upon growth in the incipient cases.

The effect in adiposis dolerosa, von Recklinghausen's disease, various dyspituitarisms, and allied conditions cannot be stated at this time.

As to the Type of Tumor.—The favorable results in cysts has already been commented upon. The adenomas should be divided into two groups: those which have grown out of the sella and become intracranial, and those still intrasellar. In the former group, Hirsch reports upon 13 cases in which the local pressure was relieved with considerable improvement of vision and at times amelioration of certain general symptoms, but for the most part the operation may be described as palliative.

In the second group, in which are often found the early acromegalies, there are often no typical signs and symptoms; atypical evidences, however, have caused a resort to the *x*-ray, and the bulging sella with a narrow outlet has confirmed the diagnosis. This is the type of case offering the greatest possibility of cure if attacked promptly. This possibility is emphasized by the brilliant result obtained by Halstead of this Association, in whose patient suffering from an adenoma there has been no return of the tumor after over three years. The malignant tumors are not common, but when they do occur the outcome is, of course, fatal.

GENERALIZATIONS

Generalizations make no claim to absolute correctness: they only mark the stages of our progress, and what is said today may be proved incorrect tomorrow. They may, however, direct the lines of advance.

Choice of Operation: Intra-cranial versus Transphenoidal.—At the present time one cannot say definitely which procedure will be the one of choice. Certain general principles can be laid down, however, and the lines of future work suggested.

The transphenoidal route cannot, unfortunately, be rendered absolutely aseptic, therefore, meningitis is always a possibility, although that menace is growing less as our experience accumulates. This route

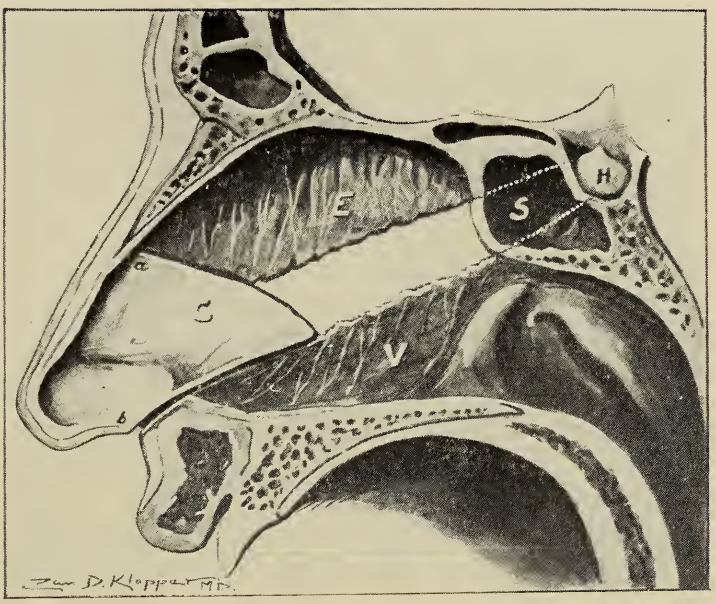

Fig. 3. Technic (Author's method).

Fig. 4. Stanley W., one year after operation.

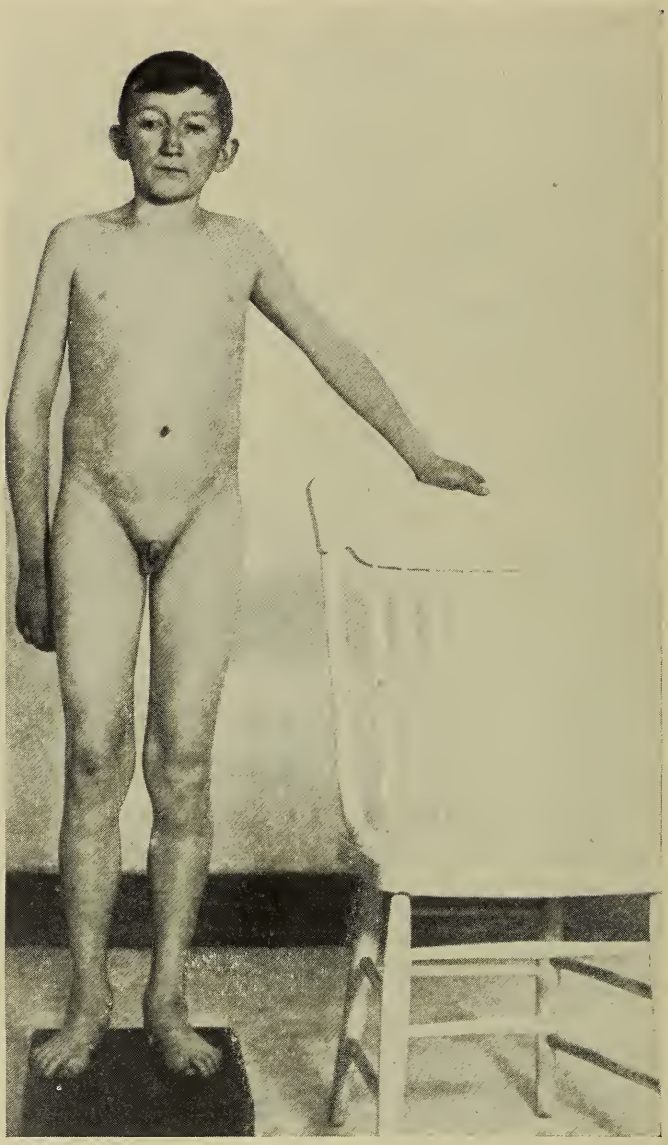

Fig. 5. Stanley W., one year after operation.

is not available for tumors lying in juxtaposition to the hypophysis and hence giving hypophyseal symptoms. Moreover, one cannot remove all the tumor when it has extended widely outside of the sella turcica.

The intracranial route is not feasible for tumors confined to the sella turcica. It can be only palliative in a majority of cases, since it does not become the operation of choice until the tumor is far advanced, and in most cases it will be impossible to remove the part of the tumor lying in the sella turcica. Therefore we may say that tumors lying for the most part in the sella turcica should be reached by the infranasal route.

Cysts may be reached and drained by either route, but more permanent results may be hoped for from the transphenoidal route, since this affords room to grow and makes subsequent puncture and drainage easy.

Tumors that have surely grown beyond the sella turcica can be reached by the intracranial route. Neighborhood tumors may be reached in the same manner.

In this connection it should be remembered that the transphenoidal route gives a decompression opening in case of future growth, for the local structures, as well as for the brain, while the intracranial in a majority of cases gives relief for general brain-pressure only. In general, then: In old cases in which palliation only is sought the intracranial route is the operation of choice; in those in which we hope for permanent results we must use the transphenoidal, although the danger of infection is greater. It is to be hoped, however, that the future may suggest some intracranial route that will offer permanent relief with a minimum of danger.

Choice of Individual Operation.—Not enough oper-

ations have been attempted by the intracranial method to choose between them. Horsley has apparently had good results with the approach through the middle fossa, although his immediate and ultimate results have not been published. The ingenious method of our able president, Dr. McArthur, or that method combined with a preliminary Krause flap, would seem to offer many advantages in extensive growths.

Of the transphenoidal methods, an experience with three cases leads me to reiterate the statements previously made, namely, that the infranasal approach as described or some of its modifications (Halstead, Hirsch and Cushing) should be the operation of choice. While all credit should be given to Schloffer, von Eiselsberg, Hochenegg, and others who have operated earlier by the supranasal route, when one contrasts the ease and simplicity of the infranasal approach with the mutilation, difficulty, and added danger of infection of the supranasal, the latter will surely be abandoned.

Concerning the modifications of the infranasal route: The labio-buccal incision of Halstead has given a most brilliant result in the hands of its originator and others. It has the advantage of leaving absolutely no scar upon the face, and the disadvantage of having the incision and the operative field in the mouth. To avoid the increased danger of an aspiration pneumonia, Halstead has given his anesthesia through a tracheotomy wound.

Making the incision in the crease under the nose has the advantage of having a field entirely outside of the mouth and the disadvantage of the scar, although in four cases which came under our observation this was invisible to ordinary inspection.

The operation of Fien is unnecessarily extenisve.

The operation of Hirsch should receive most serious consideration. In those cases in which the tumor is

small or a cyst is present, it is probable that the submucous operation can be done in stages with a minimum of danger. Yet where the nose is small or where we wish to remove as much of the tumor as is possible, a more extensive field will be found advisable. We would suggest in certain cases the possibility of cutting the basal attachment of the septum of the external nares and turning this aside. This would add materially to the room.

It is possible, of course, to add the submucous principle to any of the infranasal procedures.

Indications for Operation.—The indications for operation in cases of hypophyseal disease may be classified as absolute and relative. Operation is absolutely indicated in those cases in which the tumor immediately jeopardizes the life of the individual or is causing progressive blindness. It is relatively demanded when, because of perverted, or increased, or decreased secretion, the well-being and, ultimately, the life of the individual is threatened. Therefore, if the tumor is growing rapidly or has grown to a large size, or if a cyst has suddenly filled, we shall have pressure-symptoms, both locally and generally, that will imperatively demand intervention. As the safety of the operation increases, the field of the relative demand will be broadened.

Just how far this will extend cannot be fully stated now when our knowledge of the perverted states is limited. Thanks to the magnificent and painstaking work of Cushing, we have a basis at least upon which study may be made.

In general, it may be stated that the adipose genital type of disease where an enlarged sella can be shown, should be subjected to operation and fed with the whole gland. This is particularly true since so many of these cases, as shown by Lewis in his careful study,

are associated with cysts, in which operation is most successful.

Acromegaly should be treated most conservatively, particularly since many of the cases have passed over into the stage of hypopituitarism. As the safety of the surgical procedures grows, operation should be attempted in the early stages of the disease before the growth has left the sella. The more complicated and late cases and those with atypical signs should be treated medically until the results in the typical cases are well established.

DISCUSSION

Dr. Charles H. Mayo (Rochester, Minn.): I would like to ask the essayist if the *x*-ray has been of much beneficial value in the cases of spinal trouble that present the history and general condition found in tumors of the hypophysis. It seems to me that in the one case I saw (Dr. Mixter's) it showed the cyst very well.

The essayist has made a clear presentation of this matter and without undue enthusiasm has advocated operation for possible trivial conditions. I have not performed any of these operations myself, but have witnessed a number of them in other clinics. In watching the operation it has seemed to me impossible that a surgeon could locate, accurately, the various parts of the enlarged hypophysis when he reached it. In three cases the surgeon was content to remove the adenoma or cyst, or he was content to take out a piece of tissue not larger than a grain of wheat, to have it examined under the microscope. Decompression was accomplished in the most dangerous area so far as the operation was concerned, following that glandular therapy was repeated.

Dr. L. L. McArthur (Chicago): Once in a while it will come to the general surgeon that tumors situated in the anterior fossa produce cerebral-compression symptoms and optic-nerve disturbances that will make it necessary for him to endeavor to remove the cause. Such a call coming to me led me to make, a new route to the pituitary neighborhood by the removal of the frontal plate of the frontal bone with its orbital plate attached (doing a temporary resection), placing the piece in warm salt solution and keeping it there until such time as one could complete his work and replace it. Three such operat-

ive procedures led me the first time to a very large cyst, well out of the sella turcica, as suspected by the *x*-ray picture, and that cyst was opened and drained anteriorly. The second time it led to an infiltrating endothelioma, so pronounced by the pathologist, which had evidently sprung from the pituitary body or its immediate neighborhood. The third case was likewise a cyst, which was opened and drained from this position. The first, a very large cyst, was as large as an English walnut. In attempting to enucleate it or separate it from the surrounding structures it ruptured, and a silk-worm gut drain was introduced no further attempt at curretement being made. The patient succumbed to the shock of the surgical procedure within twelve hours. The other two patients lived, one for a year, but died finally with gradual increase of the endothelioma; the second one with partial improvement in the vision of one eye and with no improvement of the opposite side, with a reduction of the headaches, but no change in the growth. I believe that a narrow field is offered through supra-orbital resection for treating a few of the tumors of the pituitary too large and making too many neighborhood symptoms to hope to be relieved by the inferior route; but that the majority of them, if attacked at all, should be attacked through the intranasal route.

Dr. Kanavel (closing the discussion): There is no question but that the use of the *x-ray* is beneficial in such cases and of great value, particularly in the young individuals, since it is manifest that a tumor growing in young individuals will produce greater deformity than later in life after the bones are formed. It is possible by a good *x*-ray picture to find the erosion of the sella turcica. On the other hand, in many cases it is not possible, and the absence of such erosion or destruction does not necessarily prove that the patient does not have a tumor of the sella turcica or a brain tumor.

The therapeutic use of the *x*-ray has been lauded by certain men. Theoretically, it would seem to be questionable, but if these patients have improved following the use of the *x*-ray it is the proof of the pudding.

As to the so-called decompression operation: What are we operating for? We are operating to relieve local pressure on the nerves of the eye, in which case there is no question but that simple decompression will give some relief, although not the measure of relief afforded by partial removal of the tumor. If you expect to do the patient some good, you must remove a considerable portion of the tumor mass. The removal of a considerable portion of the tumor is attended with more dan-

ger than a decompression operation. That is manifest to all. At the present time, in cases of adenomas, where they are producing symptoms and operation is imperatively demanded, a capable surgeon should remove a considerable portion of the tumor, if possible. As our experience increases, we should select those cases of acromegaly in which there are beginning symptoms. We should remove a certain portion of the tumor, because it is manifest that we cannot expect to get results unless we do so. It may be reasoned that the decompression operation will allow the tumor to bulge and the posterior lobe to functionate, which it may not do under the pressure previous to decompression, but for a real result you must remove a considerable portion of the tumor.

BILATERAL CEREBRAL ABSCESS INVOLVING THE MOTOR AREAS

WARREN A. DENNIS, M. D.

ST. PAUL, MINNESOTA

The case which forms the basis of this paper was thought to be worthy of report for the reason that it presented an abscess in each cerebral hemisphere, the two about equal in size and both involving the motor areas to such an extent as to produce a practically complete bilateral hemiplegia. Cases of symmetrically located abscesses of large extent have, no doubt, occurred before, but in a fairly careful search through the literature I have been able to find only one such[1]. In this case the abscesses were secondary to double otitis media and were located in the temporosphenoidal lobes. The smaller contained three and one-half ounces of pus. They were operated upon at different times with an interval of six months between the two operations, and the surgeon had the satisfaction of having his patient recover, with special senses and mental power intact.

As in most cases of multiple abscess of the brain, the one to be reported was embolic in origin, the infecting focus being a left-sided localized empyema communicating with a bronchus. Virchow, in 1853, first called attention to the fact that septic processes involving the thoracic organs, especially the lungs, are a fruitful source of brain abscess; according to Hektoen and Riesman[2] about one-half of the cases are referable to this source, and statistics are fairly uniform

in ascribing about one-third of the cases to otitis media, while direct trauma is responsible for from 17 to 24 per cent, or even 50 per cent, according to different authorities. Judging from the cases reported throughout the literature, the estimate of 25 per cent due to embolic processes from the thoracic cavity would seem to be rather high.

Clayton[3], in 1901, collected from the literature 58 cases secondary to lung diseases and found them distributed as to primary causes as follows:

1.	Bronchiectasis	20 cases
2.	Empyema	10 cases
3.	Purulent bronchitis	9 cases
4.	Gangrene	7 cases
5.	Tuberculosis	5 cases
6.	Lung abscess	3 cases
7.	Pneumonia	2 cases
8.	Gun-shot wound of lung	2 cases

The explanation for the prominent rôle played by septic lung conditions in the causation of brain abscess is of course found in the fact that emboli escaping from the radicles of the pulmonary vein pass at once into the systemic circulation, instead of having first to run the gauntlet of the pulmonary circulation, as is the case when originating elsewhere. It is a striking fact that emboli secondary to purulent lung infections have a predilection for lodgment in the brain. Those arising from a left-sided endocarditis, on the other hand, show a preference for the kidney or spleen over the brain by a ratio of six to one. No satisfactory explanation for these peculiarities has been offered, though some have been attempted.

Brain abscess due to embolus, while it may occur in any part of the organ, is most likely to be found in the course of the artery of the fissure of Sylvius. That from otitis media is usually in the temporosphenoidal

lobe or the cerebellum, while that due to traumatism is most often found near the site of the injury.

The case to be reported is as follows:

H. B., a young man of eighteen years, in very bad general condition, was referred on February 2, 1912, from the medical to the surgical service in the University Hospital, with the diagnosis of a localized left-sided pyopneumothorax having a communication with a bronchus. The history and symptoms corresponded with this condition, and on February 5th three inches of the 8th rib were resected and several ounces of thick foul-smelling pus evacuated, and the cavity, which was found to involve the lung substance, drained. Following the drainage the temperature, which had been high (102-103° in the evening and subnormal in the morning) quickly dropped to normal, and the offensive expectoration, which had been very profuse, greatly diminished in amount. On February 11th, six days after the drainage-operation, vomiting of a large amount of green, watery fluid occurred, and this was repeated one or more times each day for a week. After the disappearance of this symptom the general condition improved greatly, and the patient was up and about, still troubled somewhat by coughing, but usually with only slight expectoration. The discharge from the wound was still profuse.

On February 24th, nineteen days after the operation, the patient had an aphasic attack characterized by a fixed stare and the repetition of the word "water" or "what." He seemed unable to form a sentence, but there was no paralysis, cyanosis, or dyspnea. The next morning he felt "fine," as he said, but in the afternoon he had a second similar attack lasting about a minute.

On February 27th, in the course of a similar attack, there was rigidity of the right hand. The diagnosis of cerebral abscess was made at this time. The pa-

tient lay usually with the head on the left side. On the next day he had another attack of aphasia and appeared paralyzed on the right side. Following this he talked intelligently for a few minutes and ate his dinner. Soon afterward he became aphasic, and there was twitching of the fingers of the right hand. The arm was held stiff. Half an hour later there was a general convulsion, the whole body twitching for about four minutes, during which time he was wholly unconscious. In three hours he was much better and fully conscious. He answered questions readily and could move the right arm and leg. The only thing complained of on the 29th was headache on the right side of the head. The patient was bright and talked normally and had no paralysis. The neurologist, called in consultation on the 28th, made the following notation: "The history of aphasic attacks, the convulsion of this morning, the turning of the head and eyes to the left, the right hemiparesis with recovery, the deviation of the tongue to the left, together with the history of pulmonary abscess, indicate a probable abscess or beginning breakdown in the left temporal lobe. If other conditions are favorable an operation to explore the left temporal and inferior parietal lobe is advised." Other conditions did not look very favorable and operation was delayed for a few days, whereupon other symptoms developed. On March 4th the left hand felt numb. On March 5th there was jerking of the left arm lasting about five minutes, and on the next day it was paralyzed. Later in the day he had a general convulsion, more marked on the right side, and followed by paralysis of the whole body. Still later in the day he could move both the left arm and leg, and still later the arm became paralyzed. Two days later he could use it again. Headache was often complained of, and vomiting was not infrequent. Involuntary urinating occurred often. The tempera-

ture was subnormal much of the time, and the pulse varied from 60 to 90. There was a double optic neuritis.

On March 16th an osteoplastic flap was made over the motor area on the left side, and at the depth of about 2 cm. an abscess containing one ounce and a half of rather thick, offensive pus was located and drained. The abscess lay beneath the ascending frontal convolution and extended downward and backward for about 3 cm.

The following day the temperature was normal, and the pulse 86. He could move both legs and the right hand, but the left hand was paralyzed. He was unable to speak, but evidently understood what was said to him.

On the 19th the pulse suddenly jumped to 160, and from this time until his death, on the 30th, the condition of the patient was continuously critical, although the pulse at one time came down to 120. The right arm was used often for purposeful actions, but the left not at all. The right pupil was dilated.

The diagnosis of probable abscess in the motor area on the right side was made, and the advisability of a second operation on that side was considered, but it was not done on account of the bad condition of the patient and a contrary opinion by the consulting neurologist, who on March 23d expressed the opinion that the unsatisfactory condition was due to the "extension of pus toward the base."

At autopsy the drained abscess-cavity was found to be almost completely obliterated, only a small amount of pus being still present beside the drainage-tubes. In the corresponding area on the right side was an abscess, rather larger than the original one and apparently composed of two smaller ones that had melted together. In the silent area in the anterior pole of the left frontal lobe near the median line was a third

abscess the size of a quarter. Each abscess had a distinct limiting capsule. The meninges and the ventricles showed no involvement.

Aside from the bilateral symmetrical character of the lesion and its involvement of the motor areas, the chief point of interest in the case concerns the question as to whether the left-sided paralysis could have arisen from the abscess in the left motor area.

It is well known that paralysis may result without actual destruction of any part of the motor tract. This fact is well illustrated in the case in hand, for on each side marked paralysis of the arm occurred. On the left side this cleared up and twice recurred before the final and permanent complete involvement. Symptoms of this character are due to what Krause[5] calls "neighborhood" involvement and may be the result of pressure, edema, or circulatory changes, or of inflammatory involvement of a degree sufficiently mild to permit of a return to normal. Thus in large abscesses of the temporosphenoidal lobe, Macewen states that hemiplegia of the side opposite the lesion is sometimes observed and that a degree of paresis would often be found if sought for. This is doubtless due to pressure transmitted to the internal capsule, and quickly clears up after drainage of the abscess.

In a case such as that in point, however, it is difficult to conceive of an abscess producing paralysis on the same side as the lesion without extending down toward the base as far as the pons, thus involving the motor fibres of the opposite side after the crossing. Lateral ventricle or other involvement would, it would seem, almost certainly supervene to end the life of the patient before this could occur. I have not met with any such in the literature.

In this connection I may perhaps be permitted to mention a case of tumor which presented this patho-

logic paradox. The report is a personal one from a neurologist and has not been published.[5]

The patient was a male, 38 years old, who had complained of severe headache for about a year. Double optic neuritis and bitemporal hemianopsia were present. There was a late developing hemiparalysis involving the face, arm, and leg on the right side; also sensory aphasia, but no motor disturbances of speech. The diagnosis was brain tumor, involving the left motor area. Operation in this region revealed nothing but increased brain tension. At autopsy an encapsulated subcortical tumor the size of a hen's egg was found in the right parietal lobe just posterior to the central convolutions. This case would seem to indicate that pressure could be exercised by a tumor across the mid-line and downward sufficiently to exert its influence upon the opposite internal capsule, and thus produce a paralysis on the same side as the lesion.

The unusual character of the paralysis could be accounted for by abscess in the pons, and Macewen[6] states that cases are recorded in which alternate hemiplegia or even bilateral paralysis of the face and limbs were due to this cause. Krause also records a case in which paralysis of the remaining side was ascribed to this cause, though the diagnosis was not confirmed by autopsy, as this was not permitted.

In our case, however, a review of the evidence seems to show that there should have been little uncertainty as to the presence of a second abscess. The embolic nature of the infection and the manner of onset of the paralysis, in retrospect at least, appear indicative.

Because of the high percentage of cases having more than one abscess, when the lesion is secondary to a pulmonary focus, uncertainty has been expressed as to whether such a case should be subjected to operation. On the other hand Bibrowicz[7] reports three of

these cases in which a lesion could be definitely localized clinically, but operative relief was refused the patients. In all these cases the autopsy showed the abscess to have been solitary and readily accessible. Moreover, Krause asserts that the percentage of cases of multiple brain abscess, secondary to pulmonary infection has been placed too high and that the statistics of the last twenty years show that in 45 per cent of the cases the abscess was solitary. In view of these figures and the practically absolute hopelessness of the condition without surgical relief, definitely localized abscess should always be drained.

Our case differs from those of otitic origin in that the symptoms of the initial stage were masked by those of the primary condition, and there was therefore no suspicion of the real situation until the signs of aphasia appeared. In going back over the chart, however, it seems probable that the initial period commenced with the vomiting on February 11th, six days after the drainage operation on the chest. It is impossible to say whether or not the three or four emboli responsible for the different abscesses all lodged at the same time, but this seems probable because of the fact that the capsules of all three were of about the same thickness and general appearance. The difference in time in the appearance of symptoms of paralysis on the two sides, only nine days, might be accounted for by the degree of completeness of arterial occlusion by the several emboli and the rate and direction of the consequent breaking down of the brain tissue.

The case seems one of great rarity and unusual interest, but the chief conclusions to be drawn, I am compelled to confess, cast considerable reflection on its management. The first abscess should have been drained immediately upon its diagnosis in spite of the poor condition of the patient, as this could not greatly improve under the circumstances; further, courage

should have equalled conviction and led to the drainage of the abscess on the right side when its presence seemed reasonably certain, even though the third abscess in the silent area of the anterior lobe held the probability of defeat in the end.

NOTE.—Directly after the reading of this paper, Dr. L. L. McArthur called my attention to a similar perplexity in a case reported by him several years ago. In this case the symptoms indicated a lesion on one side of the head. Nothing was found at operation, and at autopsy the lesion was found to be on the same side as the paralysis.

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DISCUSSION

DR. JAMES E. MOORE (Minneapolis, Minn.): I had the pleasure of watching the case reported by Dr. Dennis. The diagnosis was comparatively easy on account of the location of the abscess. In these cases, when the diagnosis is made the prognosis is not always as encouraging as we would like to have it, particularly following the course this one did. Frequently there is more than one abscess. The multiple formation of abscesses is illustrated by another case we had in the University Hospital within the last year. The abscess arose from infection of the frontal sinus. The original abscess was opened and drained. The patient did well temporarily, but afterwards became comatose and died, and a larger abscess was found in the cerebrum. In the experience I have had in operating upon cerebral abscesses, most of them came from a diseased ear, which means great difficulty in maintaining satisfactory drainage. I have found that the drainage-tubes would slip out, and in spite of my best efforts the tendency was for some of the pus to remain and the ultimate results were often unsatisfactory.

We should do our best until we have found out what we can accomplish in these very distressing cases.

Dr. Dennis is to be congratulated on his report of this case, in which the diagnosis was absolutely correct, and the treatment was the best that could be instituted in the presence of such bad conditions to start with, the patient having a disease which is usually so fatal.

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